OBITUARY NOTICES

OF

FELLOWS DECEASED.

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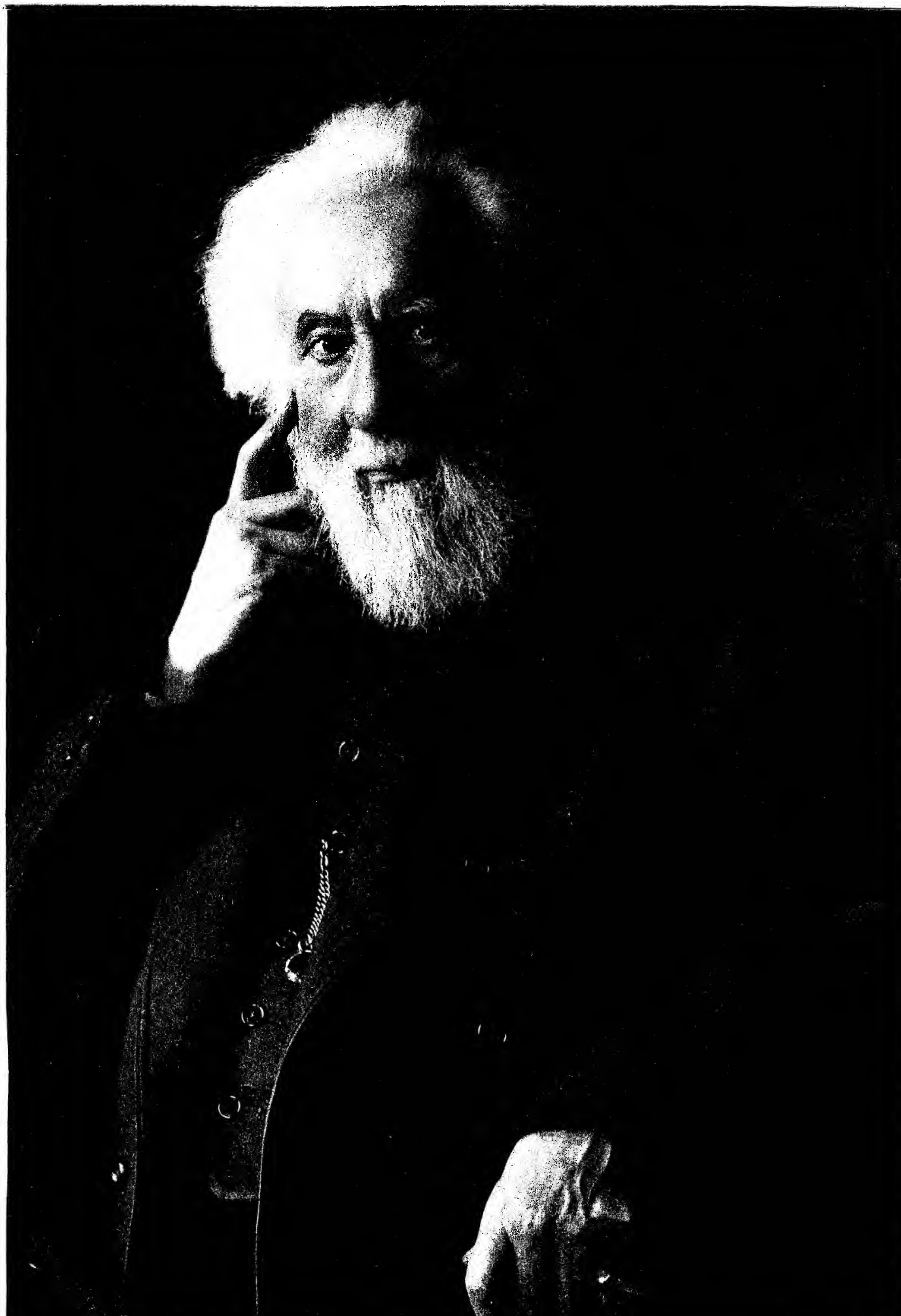

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William Huggins

SIR WILLIAM HUGGINS, O.M., K.C.B., 1824—1910.

By the death of Sir William Huggins on May 13, 1910, the Royal Society lost a past President and one of its oldest and most distinguished Fellows. He was one of the earliest to apply the spectroscope to the analysis of the light of the stars. From the importance and diversity of the discoveries which he made and the methods which he originated, he may with justice be called the founder of Astrophysics. It was his good fortune to assist in and follow the progress of this branch of knowledge for nearly half a century.

William Huggins was born in London on February 7, 1824, his father being in business in the City. He was entered at the City of London School in the first term of 1837, and stayed there till Christmas, 1843. In the school records it is stated that he gave the German Declamation in praise of the founder, John Carpenter, on Friday, July 28, 1843. After leaving school he continued his studies under private tutors, paying attention to classics, modern languages, and Hebrew. After a few years in business he was able to retire and devote his life to scientific pursuits.

In an autobiographical article in the 'Nineteenth Century,' for June, 1897, entitled "The New Astronomy: A Personal Retrospect," he tells us that it was with some hesitation that he decided to give his chief attention to Observational Astronomy. He was strongly under the spell of the rapid discoveries which were then taking place in microscopical research in connection with physiology. He joined the Royal Microscopical Society in 1852, and it was not till 1854 that he became a Fellow of the Royal Astronomical Society. He purchased a house at Tulse Hill, which was then a little distance out of London, and in 1856 built an observatory in his garden, reached by a passage from the house.

The observatory consisted of a dome, 12 feet in diameter, and a transit room. A 5-inch telescope by Dollond, equatorially mounted, and a small transit were the instrumental equipment. At that time a 5-inch refractor was considered a large rather than a small instrument. With the 5-inch equatorial Huggins made observations and drawings of the planets. His earliest published paper contains drawings of Jupiter for October 14 and 15, 1856, and of Mars for April 28 and May 19.

In 1858 he purchased from Mr. Dawes an object glass of 8 inches diameter made by Alvan Clark, the famous founder of the American firm which in later years made the great telescopes for Washington, Lick, and Yerkes. This object glass had met with high approval from Dawes, and no one was more competent to judge than that distinguished observer of double stars, who obtained the name of "eagle-eyed" by reason of the acuteness of his vision. Huggins thus became the possessor of a very

fine telescope, which he mounted equatorially and provided with slow motions.

He tells us that he soon became dissatisfied with the routine character of ordinary astronomical work. The belief that the most important and interesting discoveries in astronomy had been made, and that details only required to be filled in, was prevalent in the middle of last century. That this view was entirely erroneous has been proved as regards Dynamical Astronomy by the work of Delaunay, Hill, and others, and as regards Stellar Astronomy by the recent cosmical developments which have their foundation in the accurate measures of the positions of stars.

But neither of these main branches of the astronomy of the period was quite suited to the genius and tastes of Huggins. "Just at this time," he tells us, "when a vague longing after newer methods of observation for attacking many of the problems of the heavenly bodies filled my mind, the news reached me of Kirchhoff's great discovery of the true nature and the chemical composition of the sun from the interpretation of the Fraunhofer lines. This news was to me like the coming upon a spring of water in a dry and thirsty land. Here at last presented itself the very order of work for which in an indefinite way I was longing—namely, to extend his novel methods of research upon the sun to the other heavenly bodies."

Kirchhoff's memoir, in which he demonstrated with full experimental verification the relationship of the dark lines in the solar spectrum to the bright line spectra of the elements obtained in his laboratory, was published in 1859. In the following year, in conjunction with Bunsen, he showed that the sun contained many elements known to exist on the earth. Although the theoretical ideas of Kirchhoff were not so novel as he had thought, having been to some extent largely anticipated as regards the main principles by Stokes and Balfour Stewart, spectroscopy as an important method of research dates from this paper of 1859.

Huggins relates in the 'Nineteenth Century' how, on January 15, 1862, he attended a soirée of the Pharmaceutical Society, and met his friend and neighbour, W. A. Miller, Professor of Chemistry at King's College, London, and Treasurer of the Royal Society from 1860 to 1871. After the soirée the two friends drove back to Tulse Hill together, and Huggins asked Miller to join him in his proposed attempt to apply the method of "prismatic analysis," employed by Kirchhoff and Bunsen, to the stars. Miller at first hesitated, as the small amount of light from a star rendered the success of the experiment doubtful, but finally agreed to co-operate.

Kirchhoff and Bunsen had compared in their spectroscope, simultaneously, the light of the sun with light from various terrestrial sources. But the total light received from one of the brightest of the stars is less than the fraction 10^{-10} of that received from the sun. All the light which falls from a star on an object glass can, however, be concentrated into one point, the telescopic image. A spectroscope had therefore to be designed and applied

in such a manner that the whole of this light could be utilised. It was accordingly mounted firmly on the telescope, with the slit at the focus; and the terrestrial light with which comparison had to be made was obtained from the sparks of an induction coil, and was bent round into the tube of the spectroscope by means of a reflecting prism placed over half of the slit. The spectrum of a star obtained in this way was, however, a mere line, too narrow for the eye to detect any lines crossing it. To broaden it a cylindrical lens—an optical appliance then seldom used—was obtained and placed within the focus of the telescope, thus lengthening the point of light on the slit into a short line, and giving the necessary breadth to the spectrum.

By the end of 1862 a stellar spectroscope had been constructed of sufficient dispersive and defining power to resolve such lines as D and *b* of the solar spectrum, and competent to reveal the finer lines in the spectra of the stars if these should be found to resemble those in the solar spectrum. This spectroscope utilised two dense flint glass prisms of 60° angle. The collimating lens had a diameter of 0·6 inch and a focal length of $4\frac{1}{2}$ inches, the ratio of aperture to focal length enabling the whole of the light from the linear image of the star on the slit to fall upon the collimating lens. The viewing telescope had an aperture of 0·8 inch and a focal length of 6·75 inches; it was carried by a micrometer screw which turned it so that different parts of the spectrum could be examined, and the relative positions of the lines in the spectrum accurately measured. Particular pains were taken to ensure accuracy in the relative positions of the stellar and comparison spectra. Owing to flexure of the spectroscope the absolute reading of the micrometer was not the same for the same spectrum line for stars at different altitudes. Therefore for each star a preliminary observation was made, which secured coincidence of the comparison spectrum of sodium with that obtained from a small alcohol lamp saturated with sodium chloride placed close to the centre of the object glass of the telescope. It was verified that when this adjustment had been made, lines from other parts of the two spectra also coincided. Thus when for any star the presence of the double line corresponding to D had been satisfactorily determined, it was only necessary in further comparisons so to adjust the spark that the sodium lines from it agreed with these lines from the star.

Besides the points of difficulty which were thus successfully surmounted in the construction of a stellar spectroscope, an additional one arose from the want of convenient maps of the spectra of terrestrial elements. Huggins devoted a large part of the year 1863 to mapping, with a train of six prisms, the spectra of the elements, using as a standard of reference the spark spectrum of air obtained by the discharge of a large induction coil fed by a condenser consisting of nine Leyden jars. In this way maps and tables extending through the whole length of the visible spectrum were made for no less than 24 elements.

A brief note in which diagrams of the spectra of Aldebaran, α Orionis, and

Sirius are described had been communicated to the Royal Society in February, 1863, but it was not until April, 1864, that complete results were communicated. In this important paper ('Phil. Trans.,' 1864, vol. 154, pp. 413—435) the authors state that they have investigated the spectra of fifty stars to a greater or less extent, but have concentrated their efforts in the complete examination of two or three stars. The spectrum of Aldebaran, for example, was compared directly with the spectra of sixteen terrestrial elements, and the existence of sodium, magnesium, hydrogen, calcium, iron, bismuth, tellurium, antimony, and mercury in this star was announced. In Betelgeux they found sodium, magnesium, calcium, iron, and bismuth. The presence of some of these metals has not been confirmed by later work; but these remarkably accurate pioneer researches established beyond question the authors' conclusions that "the stars, while differing the one from the other in the kinds of matter of which they consist, are all constructed upon the same plan as our Sun, and are composed of matter identical at least in part with the materials of our system."

Simultaneously with the appearance of the first announcement by Huggins and Miller of their preliminary results, Rutherfurd published, in the 'American Journal of Science,' an account of a study by very similar methods of the spectra of the Moon, Jupiter, Mars, and several of the fixed stars. He did not, however, pursue this line of research, but turned his thoughts to astronomical photography. The work of Secchi, carried on contemporaneously with that of Huggins and Miller, was complementary to their studies. He investigated and classified the spectra of 600 and later of 4000 stars by the use of a prism placed in front of the object glass of his telescope. This method had been employed by Fraunhofer in very delicate work, before, however, the meaning of the dark lines in stellar spectra named after him was understood; and later, Donati used the imperfect method of a slitless spectroscope. The use of a prism in front of the object glass avoided the observational difficulties with which Huggins and Miller had to contend, and was suitable for the important work of a survey of a large number of stars. The disadvantage of the method is the absence of the fiducial lines which are obtained by the comparison spectrum when a slit spectroscope is used. To interpret stellar spectra with certainty the more laborious method of Huggins and Miller was necessary, more especially in the early stages of the science.

Owing to pressure of other duties, Miller was obliged to discontinue his co-operation with Huggins when their researches had reached this stage. They had worked together from January, 1862, to April, 1864, and in that period had laid the foundations of the methods to be applied in the spectroscopic study of the stars. From this time till his marriage, ten years later, Huggins pursued his researches single-handed.

On August 29, 1864, Huggins made a discovery of cardinal importance. Having pointed his telescope on a planetary nebula in Draco, described in Sir John Herschel's Catalogue as "very bright, pretty small, suddenly

brighter in the middle, very small nucleus," he found that the light of the nebula, " unlike any extra-terrestrial light which I have previously subjected to prismatic analysis, is not composed of light of different refrangibilities, and therefore does not form a spectrum. A great part of the light of this nebula is monochromatic, and, after passing through the prisms, remains concentrated in a bright line, occupying in the instrument the position of the part of the spectrum to which its light corresponds in refrangibility." A narrower slit showed two fainter lines in addition. The bright line was found to agree in position with the brightest of the air lines, viz., the strongest line in the spectrum of nitrogen; the faintest of the three lines coincided with the F line of hydrogen, while the third line did not coincide with any known line, but its position was identified by its proximity to a line of barium.

This historic observation proved that the nebula in Draco is not an incandescent solid or liquid transmitting light of all refrangibilities through an atmosphere which intercepts some of them—not a body of the type of our Sun—but is a widely extended mass of luminous vapour. In a moment a definite answer had thus been given to the question whether all nebulæ were aggregations of stars too distant to be resolved into separate units by the telescope, or were, in Sir William Herschel's words: " A shining fluid fit to produce a star by its condensation." By the middle of the nineteenth century many nebulæ had already been resolved into multitudes of stars by the large telescopes which had been directed to them, and it was matter of speculation whether all might not be resolvable with still larger telescopes. Huggins' spectroscope showed that this was not the case, and left tenable the view that nebulæ were " the early stages of long processions of cosmical events which correspond broadly to those required by the nebular hypothesis."

The researches on nebulæ were pursued by Huggins with characteristic thoroughness. Eight other bodies were found to give spectra which indicated their gaseous nature. In all of these the same bright line was found coincident in position with the line of nitrogen, while generally the two fainter lines seen in the Draco nebula were also seen. Six of these bodies were small and comparatively bright objects, designated as " planetary " nebulæ by Herschel. The other two were the ring nebula in Lyra and the nebula in Vulpecula. But the great nebula in Andromeda, and the bright condensation associated with it, did not give a gaseous spectrum like the other nebulæ examined; they gave a continuous one, similar to that shown by a star, though, on account of its faintness, dark lines could not be seen crossing it.

In the following winter the great nebula in Orion was examined, and the same three bright lines found in the spectrum of all parts of that nebula. Its gaseous constitution was therefore established. Observations of Lord Rosse and Prof. Bond were thought to have resolved the great nebula, as well as the ring nebula, into discrete points. If so, these nebulæ must be

considered not as simple masses of gas, but as systems formed from the aggregations of such masses around centres of condensation. The four bright stars in the centre of the Orion nebula were also examined; they showed continuous spectra with no indication of absorption lines, and it was noted that the continuous spectrum of the stars in the neighbourhood of the green nebula line was brighter than this line in the adjacent nebula. The existence of the same three bright lines in all nebulæ, indicating an identical gaseous constitution, appeared to show that the nebulæ possessed "a structure and purpose in relation to the Universe, altogether of a distinct and of another order from the great group of cosmical bodies to which our sun and the fixed stars belong." Huggins soon afterwards adopted the view that the gaseous nebulæ were to be regarded as precursors of the stars in the course of evolution of the Stellar Universe.

Leaving on one side speculative questions, he made a more complete examination of the nebulæ and clusters, comparing his results with the telescopic observations made with Lord Rosse's great reflector. By August, 1866, he had examined the spectra of more than 60 nebulæ and clusters. About one-third of these were found to give spectra consisting of bright lines, indicating their gaseous constitution. None of the bodies showing bright line spectra had been resolved into stars by Lord Rosse. On the other hand, all the true clusters, which could be resolved into distinct bright points, gave spectra which were apparently continuous, and, in addition, many nebulæ, of which the great nebula in Andromeda is a striking example, gave spectra of a similar character.

At this stage, subjects for investigation crowded themselves upon Huggins' attention. The regular fluctuation in the brightness of variable stars might be elucidated by spectroscopic examination. If physical changes occurred, they would be shown by changes in the spectra. Again, if diminution of brightness were caused by the interposition of a dark companion, additional lines of absorption might be shown in the spectrum. Observations of Betelgeux at its maximum brightness in February, 1866, showed that a group of lines was missing which had been seen and mapped two years previously. While these observations were in progress a new star appeared in the sky. Such bodies may be considered as extreme types of variable stars. They flash up suddenly and slowly fade. One of these rare phenomena was opportunely observed on May 12, 1866, by Mr. Birmingham, of Tuam, County Galway. He immediately communicated to Mr. Huggins his discovery of a new star of the second magnitude in the constellation of Corona. When the news was received on May 16 the star was of the third magnitude; it was still a bright star and suited to spectroscopic examination. Huggins sent a messenger to Miller and they directed the spectroscope to the new star.

Examination showed the spectrum to be different from any previously examined; and, as described in the 'Proceedings' (vol. 15, p. 146):—"The light of the star is compound and has emanated from two different sources.

Each light forms its own spectrum. In the instrument these spectra appear superposed. The principal spectrum is analogous to that of the sun, and is evidently formed by the light of an incandescent solid or liquid photosphere which has suffered absorption by the vapours of an envelope cooler than itself. The second spectrum consists of a few bright lines, which indicate that the light by which it is formed was emitted by matter in the state of luminous gas." The two principal bright lines were in the positions F and C of the solar spectrum, and showed that the luminous gas consisted in part of hydrogen. Their great brightness was taken as an indication that the gas was hotter than the liquid or solid photosphere of the star. In conjunction with the sudden outburst of the star and its rapid decline in 12 days from the second to the eighth magnitude, these facts suggested that the star had become suddenly enwrapt in the flames of burning hydrogen.

The early researches of Huggins are of such general character as to make an appeal to all who are interested in physical science as well as to astronomers. The observations of the new star astonished a still wider circle, which did not hesitate to conclude that, "from afar astronomers had seen a world on fire go out in dust and ashes." The new star drew attention in a sensational manner to the possibilities of the new science of which Huggins was laying the foundations.

The structure of comets is a subject marked out for elucidation by prismatic analysis. In 1864 Donati found that the comet of that year had a spectrum consisting of bright lines. Huggins examined faint comets which appeared in 1866 and 1867, and was able to detect a very faint continuous spectrum from the coma, from which he inferred that its light was probably reflected sunlight. In the middle of this faint spectrum a bright point was seen, showing that the nucleus of the comet was self-luminous and gaseous. This bright point appeared to coincide with the principal bright line in the spectra of nebulæ, and at first Huggins supposed the lines might have the same origin. Early in 1868 he observed the spectrum of Brorsen's comet, and found it to consist of three bright bands. The transverse length of the bands showed that they did not arise solely from the nucleus of the comet but from the brighter parts of the coma. When the slit of the spectroscope was narrowed the bands did not become sharp, and in that respect differed from the spectra of nebulæ.

The positions of the bands were determined by micrometric measurements, and by comparison with various terrestrial spectra. It happens, curiously, that this earliest obtained spectrum of a comet's head is not of the character usually found in these bodies; but it has recently been pointed out that the spectrum of the tail of Morehouse's comet of 1908 resembles that found by Huggins in the head of Brorsen's comet.

On June 13, 1868, a comet was discovered by Winnecke, with a coma very bright at the centre and suitable for spectroscopic observation. Huggins examined it with a spectroscope containing two 60° prisms, and found that the light was resolved into three bright bands. These bands were brightest

on their less refrangible sides, where they commenced sharply, and gradually faded away on their more refrangible sides. They could not be resolved into lines even when a spectroscope of greater dispersion was employed. On the following day the spectrum as drawn and measured was compared with various terrestrial spectra. The positions of the bands, and their general character, resembled the spectrum obtained when a spark is passed through olefiant gas. The next evening a direct comparison was made between the spectra of the comet and of olefiant gas, and the bands were found to agree exactly in position. Later observations have shown that this spectrum of the three bands found in hydrocarbons is characteristic of the heads of comets.

Huggins delivered at the British Association at Nottingham on August 24, 1866, a "Discourse on Spectrum Analysis applied to the Heavenly Bodies." He concludes the lecture by summing up the knowledge which had been gained by the method. This summary serves to show the great discoveries he had made in the few years from 1862 to 1866 and the state of Astrophysics at the time.

1. All the brighter stars, at least, have a structure analogous to that of the Sun.

2. The stars contain material elements common to the Sun and Earth.

3. The colours of the stars have their origin in the chemical constitution of the atmospheres which surround them.

4. The changes of brightness of some of the variable stars are attended by changes in the lines of absorption of their spectra.

5. The phenomena of the star in Corona appear to show that in this object at least great physical changes are in operation.

6. There exist in the heavens true nebulæ. These objects consist of luminous gas.

7. The material of comets is very similar to the matter of gaseous nebulæ and may be identical with it.

8. The bright points of the star clusters may not be in all cases stars of the same order as the separate bright stars.

Observations made later, in 1868, showed the incorrectness of the view (7) that cometary matter was similar to that of the nebulæ. The explanation of the colours of stars cannot now be accepted as complete. With these exceptions the conclusions of this lecture all hold, and are a statement of the important and fundamental knowledge then obtained by the spectroscopic study of the stars.

At the time when he was making simultaneous comparisons of stellar and terrestrial spectra for the determination of the chemical constitution of the stars, Huggins realised that such comparisons might serve to determine the motions of the stars in the line of sight. If the stars were moving to or from the Earth, their motion compounded with the Earth's motion would alter to an observer on the Earth the wave-length of the light emitted by them, and consequently the lines of terrestrial substances would no longer coincide in

position in the spectrum with the dark lines produced by the absorption of the vapours of the same substances in the stars. The two-prism spectroscope he employed was sufficient to show that no displacement in any of the stars he examined was so great as the interval between the two D lines, and thus to obtain the important conclusion that none of these stars had velocities to or from the earth amounting to 196 miles per second. From the velocities of the few stars whose parallax was sufficiently well known, it was seen that the order of the quantity to be sought was in their case only a fraction of the interval between the D lines. He therefore designed and had constructed a spectroscope of much greater dispersion, equivalent in power to $6\frac{1}{2}$ prisms of 60°. At the same time he experimented in different ways with a view to making the comparison spectrum more trustworthy and more convenient. Two pieces of silvered glass were fixed before the slit at an angle of 45°, leaving an opening of one-tenth of an inch between them for the passage of the star's light from the object glass of the telescope. The light from an induction-spark whose position relatively to the telescope was kept fixed by careful precautions, was reflected into the spectroscope by these pieces of glass. The star's spectrum was therefore seen accompanied by two comparison spectra, one on each side of it.

In the winter of 1867 he made observations on Sirius, a star which was suitable on account of its brilliancy and of the great intensity of its hydrogen lines. He compared the line at F with the corresponding hydrogen line. At first hydrogen at atmospheric pressure was used, but as the width of the line obtained in this way was greater than the corresponding dark line in the spectrum of Sirius, a vacuum tube fixed in front of the object glass was substituted. The line thus obtained was about one-fifth of the width of the broad line in the spectrum of Sirius, and was seen distinctly as a bright line on the dark line. It was clearly seen not to coincide with the middle of the line, and the distance from the middle was estimated in terms of the micrometer screw. It was found that the displacement towards the red was 1·09 tenth-metres, giving a velocity of separation of Sirius from the Earth of 41·4 miles per second. Allowing 12 miles for the recession of the Earth from Sirius owing to its orbital motion, there remained a movement of recession of 29·4 miles per second of Sirius from the solar system.

The importance of these researches and their extreme delicacy made it desirable that they should be pursued with greater optical power. The President of the Royal Society, in his address for 1869, stated that "the Council have resolved to provide a telescope of the highest power that is conveniently available for spectroscopy and its kindred inquiries. . . . The instrument will be entrusted to such persons as, in their opinion, are the most likely to use it to the best advantage for the extension of this branch of science; and in the first instance there can be but one opinion that the person so selected should be Mr. Huggins."

The Oliveira bequest of £1350 to the Society facilitated this project, and a tender from Mr. Grubb was accepted in April, 1869, for the construction of

an object glass of 15 inches aperture and of 15 feet focal length, on an equatorial which could be easily worked by an observer without an assistant. In order that observations on the heat of the stars, which had already been attempted by Huggins, might be pursued, the equatorial, at the suggestion of de la Rue, was also provided with an 18-inch reflector which could be substituted for the 15-inch refractor.

A drum of 18 feet diameter was erected in 1869–70, instead of the 12 feet dome, to house the new equatorial. By February, 1871, the instrument was installed and found to work admirably. Spectroscopes specially adapted for the instrument were constructed suitable for the observation of stars, nebulæ, and the sun.

With this new instrument the determination of velocities in the line of sight was immediately continued. The best means of introducing the comparison spectrum were again considered. Although the reflection from a silvered surface in front of the slit worked well, some troublesome adjustments and a liability to displacement were avoided by a plan adopted instead of it. Holes were drilled in the telescope tube 2 feet 6 inches from the focus, and tubes carrying the vacuum tubes or electrodes were inserted. The spark was in this way brought right into the axis of the telescope.

The following paragraph from a paper presented to the Royal Society in 1872, in which the results of observation of thirty stars are given, may be quoted as showing the great difficulties of these observations and the care bestowed on them by Huggins:—"It may be well to state some circumstances connected with these comparisons which necessarily make the numerical estimations, given farther on, less accurate than I could wish. Even when spectroscope C, containing four compound prisms, and a magnifying power of 16 diameters are used, the amount of the change of refrangibility to be observed appears very small. The probable error of these estimations is therefore large, as a shift corresponding to 5 miles per second (about 1/40 of the distance of D_1 to D_2), or even a somewhat greater velocity, could not be certainly observed. The difficulty arising from the apparent smallness of the change of refrangibility is greatly increased by some other circumstances. The star's light is faint when a narrow slit is used, and the lines, except on very fine nights, cannot be steadily seen, in consequence of the movements in our atmosphere. Further, when the slit is narrow, the clock's motion is not uniform enough to keep the spectrum steadily in view; for these reasons I found it necessary to adopt the method of estimation by comparing the shift with a wire of known thickness, or with the interval between a pair of close lines. I found that, under the circumstances, the use of a micrometer would have given the appearance only of greater accuracy. I wish it therefore to be understood that I regard the following estimations as provisional only, as I hope, by means of apparatus now being constructed, to be able to get more accurate determinations of the velocity of the motions."

Comparison with later measurements has shown, as is not surprising when it is considered that all the apparatus had to be improvised, that the velocities obtained for these thirty stars are, when regarded as standard determinations, of but small weight. Nevertheless, the result of the efforts which Huggins made to determine precise values for the velocities in the line of sight must be considered as a great step in the progress of Astronomy. He attempted a practical problem so novel and difficult that only his appreciation of its great importance could have sustained him in his effort. After the publication of Huggins' paper, line of sight determinations were taken up at Greenwich, and observations were carried on for many years by Mr. Maunder. But it was not till the years 1888 to 1892, when photography was applied by Vogel and Scheiner, that a reliable procedure was evolved.* Huggins had the good fortune to witness that immense development of this branch of Astronomy, in which he was the pioneer, which has been made possible by the great light-gathering power of large telescopes combined with spectroscopes of high resolving power and sensitive modern photographic plates.

The large spectroscope which he had installed in 1867 enabled Huggins to prosecute researches on the Sun. A comparison of the spectrum of the Sun near its limb with that at its centre, and of the spectra of sunspots with the solar spectrum, engaged his attention. He also endeavoured to obtain in direct sunlight by increased prismatic dispersion the spectrum of the prominences which are visible during total eclipses on the limb of the Sun, and in the Report of his Observatory to the Royal Astronomical Society in February, 1868, described fully and definitely the method of observation. In the detection of the spectrum of the prominences he was, however, anticipated by Janssen and by Lockyer. But he had priority a few months later in a new application of the principle that was involved; he succeeded in making an image of a solar prominence visible in each bright line by the simple method of widening the slit of the spectroscope.

In 1875 Dr. Huggins married Miss Margaret Lindsay Murray, in whom he found, to quote his own words, in addition to an inspiring helpmate, an able and enthusiastic assistant. Her name is associated with that of her husband in the authorship of most of the work after this date.

From 1876 to 1880 Huggins was engaged on the application of photography to stellar spectroscopy. So early as 1863 he had obtained a spectrum of Sirius on a wet collodion plate, but did not prosecute researches in this direction, as the plates were not sufficiently sensitive, and their use involved numerous practical difficulties. He saw that the gelatine dry plate, from the convenience attending its use and its great sensitiveness, was well adapted for spectroscopic research. Its reaction to ultra-violet light made it possible for spectra of stars to be obtained in this hitherto unexplored

* H. C. Vogel, "On the Progress made in the Last Decade in the Determination of Stellar Motions in the Line of Sight," 'Astrophys. Journ.,' vol. 11, p. 373.

region. But, owing to the great absorption by glass of light of wave-length shorter than the violet, a refracting telescope and glass prisms are unsuitable for work on this part of the spectrum. Huggins therefore decided to use the 18-inch speculum mirror of his Cassegrain telescope and to fit it with a suitable spectroscope. This he constructed of a single 60° prism of Iceland spar, with collimator and camera lenses of quartz. He removed the small convex mirror of his telescope and mounted the spectroscope with its slit at the focus of the 18-inch speculum, 13 feet distant from the eye-end of the telescope. To enable him to bring the star to be observed on the slit, and to keep it exactly there during an exposure which might be of an hour's duration or more, he made the jaws of the slit of speculum metal and maintained the adjustment by watching the reflection of the star-image from the jaws by means of a small telescope mounted in the central hole of the large speculum.

With this instrument he obtained in 1876, with the assistance of Mrs. Huggins, a spectrum of Vega showing seven strong lines, two of which were known to be due to hydrogen, and the remaining five were continuations hitherto unobserved of the same series of hydrogen lines, which afterwards became classical as the subject of Balmer's law (*infra*). With some minor improvements, observations on the brighter stars were continued in this manner till 1879, when the results were communicated to the Royal Society in a paper on "The Photographic Spectra of Stars" ('Phil. Trans.,' 1880, Part II, p. 669).

Maps and tables of wave-lengths of the lines are given for the white stars Sirius, Vega, γ Cygni, γ Virginis, η Ursæ Majoris, γ Aquilæ, and (a star of different type) Arcturus, extending from H_γ, λ 4340, to λ 3300. The spectra of these white stars were found to possess a remarkable similarity. The photographs showed 12 very strong lines. The first three of these were the hydrogen lines, H_γ, H_δ, H_ϵ. The remaining nine were not coincident with any strong lines in the solar spectrum, but the symmetrical appearance of the whole group suggested at once that all belonged to the spectrum of the same substance. These lines were afterwards obtained in the spectrum of terrestrial hydrogen by Cornu. In 1885 Balmer showed that all were embraced with great exactness in the simple formula $\lambda = 3645{\cdot}6\, m^2/(m^2-4)$ by giving m in succession the values 3, 4, 5, etc. Huggins afterwards obtained spectrographs of stars showing 31 lines belonging to this series with the same exactness; and thus the discovery of the hydrogen series in the spectra of the stars provided the stimulus for the subsequent sorting out of the lines of the spectra of other elements into series which has been effected by Kayser and Runge, Rydberg, Ritz, and other physicists.

Huggins directs attention to three characteristics in these stellar spectra: (i) the differences in width and diffuseness of the hydrogen lines; (ii) the absence or presence of the K line due to calcium and its intensity relatively to the hydrogen lines; and (iii) the number and distinctness of other lines. These features served as a basis for a classification of the stars. In an

addendum to the paper, in which spectra of Capella, Aldebaran, and Betelgeux obtained by him in January, 1880, are also considered, he arranges the stars in an order substantially the same as that given by Vogel a few years previously, as indicating successive stages in the evolution of these bodies.

The appearance of a fairly bright comet, in 1881, presented an opportunity for pushing researches on cometary spectra into the ultra-violet region. The spectrum was photographed with the spectroscope he had used for stars, and revealed two strong bright lines at 3883 and 3870, which were identified with two cyanogen lines found by Profs. Liveing and Dewar. There was also a continuous spectrum, in which the Fraunhofer lines were visible, showing that a part of the comet's light was reflected solar light. In the following year he photographed the spectrum of Comet Wells, and found it to be entirely different from the comet of 1881. It consisted of five bright bands, whose positions he measured, but he did not succeed in determining their chemical origin.

In this year he also obtained a photograph of the spectrum of the Orion nebula, and found, in addition to the lines he had observed visually long before, an extremely strong line in the ultra-violet at the approximate wave-length 3730.

In the years 1882 to 1885 a good deal of attention was given by Huggins to the problem of photographing the sun's corona without an eclipse. The photograph of the spectrum of the corona taken by Prof. Schuster in Egypt during the eclipse of May 17, 1882, had shown the coronal light to be strongest in the part of the spectrum from G to H. By the use of screens of coloured glass or liquid Huggins limited the light to this range of wave-length in the hope that this would enable the coronal light to hold its own against the atmospheric glare. He thus obtained photographs which in their general features resembled the corona, and he, as well as other experts to whom the photographs were submitted, thought that a successful start had been made. A resemblance between photographs taken in England near the time of the eclipse of May 6, 1883, and those taken by the eclipse observers in Caroline Island were held to lead to the conclusion that to a distance of 8′ from the Sun's limb the appearances on Huggins' plates were genuine pictures of the corona. A committee was appointed by the Royal Society for the purpose of carrying on experiments under the more favourable conditions provided by high elevation. Mr. Woods, an observer at the 1883 eclipse, was sent to the Riffel Alp at an altitude of 8,500 feet in July, 1884, and took a large number of photographs. Fine matter in the air—due, in Huggins' opinion, possibly to the Krakatoa explosion or else to ice spicules—was always present, and produced sufficient stray light to prevent the experiment being successful. Photographs taken subsequently at the Cape also gave negative results. A final test of the method was made at the eclipse of August, 1886, during the partial phase. In a letter to 'The Times' Huggins says: "The partial phases of this eclipse furnished conditions which would put the success of the method

beyond doubt, if the plates showed the corona cut off partially by the Moon during its approach to and passage over the Sun. As the telegrams received state that this partial cutting off of the corona by the Moon is not shown on the plates, I wish to be the first to record this untoward result. I greatly regret that the method which seemed to promise much new knowledge of the corona would seem to have failed." This problem had called for an attack, and the failure of an observer so skilful and persevering shows that a successful result can only be looked for under exceptional atmospheric conditions.

His attention was withdrawn from stellar researches for several years while he was experimenting on the photography of the corona without an eclipse. Some repairs of his instrument were also required. A number of alterations were made, of which the most important was the modification of his equatorial by which it was made to carry both the refractor and reflector instead of only one of them. In addition he constructed a new and more rigid spectroscope for visual observations. In 1888 he resumed observations, both visual and photographic, of the spectrum of the Orion nebula. The visual observations, which were continued to 1890, were concerned with the wave-length and identification of the principal nebular line. The following summary of the history of this line is given by Keeler in the 'Lick Observatory Publications,' vol. 3:—"In 1864 Huggins found that this line coincided in position with the brightest air line, a coarse double, the mean position of which is λ 5003. He then considered that it was probably due to nitrogen; in 1872 with his more powerful spectroscope he gave the wave-length as 5005, and was still disposed to regard it as due to nitrogen, ascribing its displacement to recession of the nebula from the Sun. In 1874 he found that the line was apparently coincident with the less refrangible of the nitrogen lines. He appears by this time to have abandoned his view of the chemical origin of the line. He also discovered a very convenient comparison line in the spectrum of lead. In 1887 Prof. Lockyer, in connection with the 'meteoric hypothesis,' suggested that the nebular line is coincident with the magnesium fluting at λ 5006·4. This rendered the exact determination of the position of the line a matter of great interest. In 1889 Dr. and Mrs. Huggins compared the nebular line with the magnesium fluting, using a very high dispersion. They found the line to be more refrangible than the edge of the magnesium fluting, and to be fine and sharp like the hydrogen lines. They also concluded that neither visual nor photographic observations afforded any evidence of the presence of magnesium in the nebula. In 1890 Prof. Lockyer, from a review of previous observations and his own in that year, maintained that the chief nebular line is a remnant of the magnesium fluting. Dr. and Mrs. Huggins repeated their observations, and confirmed their result that the nebular line is more refrangible than the head of the magnesium fluting." At the request of Dr. Huggins the position of the line was determined by Prof. Keeler, at the Lick Observatory, who found the nebular line to be at λ 5007·0, the head of the magnesium fluting at λ 5007·5, and the two

nitrogen lines at λ 5001·0 and λ 5005·3. These observations of Keeler showed conclusively that the nebular line is not a remnant of the magnesium fluting, and its chemical origin is still unknown.

In the 'Proceedings' (1889, vol. 46, p. 40), an account is given of a remarkable photograph of the ultra-violet spectrum of the Orion nebula taken on February 5, 1888. The photograph shows the strong line λ 3727. As the slit was set so that two bright stars in the centre of the nebula fell upon it, the photograph shows the continuous spectrum of these stars as well as the bright line spectrum of the nebula. Four groups of faint lines can be seen extending across the continuous spectra of the stars into the nebula. As the stars have these lines in common with the nebula, the conclusion is drawn that they are not merely in the same direction as seen from the Earth, but are physically connected with it. The photograph was too faint to be reproduced, but was shown to several eminent spectroscopists, who agreed that the appearances on the negative were real lines. In particular, Prof. Hale, in 1894, examined the photograph and verified the existence of the faint lines, and stated that the increase in width and brightness of the lines where they crossed the spectra of the stars was most striking. In the year 1890 the two lines H_ζ and H_η in the hydrogen series were discovered in the spectrum of the Orion nebula, as well as a line λ 3868.

In the same year, on a very clear night in September, Huggins obtained photographs of the spectrum of Vega to determine the point at which the star's light is extinguished by the absorption of the earth's atmosphere. The light was found to be abruptly weakened at λ 3000, but to be faintly seen to λ 2970. Similar results were found from observations of the Sun's spectrum.

In 1890 he discovered a series of six broad lines in the spectrum of Sirius in the extreme ultra-violet from λ 3338 to λ 3199. In the same year he made visual observations of the positions of the bright band in the blue in Wolf-Rayet stars, confirming observations of Vogel's, that this band did not coincide with the blue band due to carbon which is seen in a spirit lamp flame.

The appearance of Nova Aurigæ in 1892 naturally attracted the attention of Dr. and Mrs. Huggins. They observed the remarkable phenomenon that the bright hydrogen lines and some others were doubled by a dark line of absorption on the blue side, and they estimated the relative shift to be 550 miles a second, a result in accord with the estimates of other observers. They also saw in the spectrum the double line of sodium. By comparison with suitable spectra they found that, at this stage of the star's history, there was no sign of the nebular line, and no relationship with cometary spectra. With the ultra-violet spectroscope they obtained a photograph of the spectrum up to the extreme limit which the absorption of the atmosphere permits. It shows the hydrogen series in bright lines, with accompanying absorption lines on the blue side, as well as a large number of other lines. They were unable to examine the spectrum very completely after its

remarkable change into that of a planetary nebula in August, announced by Campbell, owing to alterations which were being made in their telescope. A few observations on the character of the bright bands in the positions of the principal and second nebular lines were made by them in February, 1893, and they laid some stress on the difference between these bands and the sharp lines in the spectra of nebulæ.

Huggins favoured some connection between the later stages of new stars and planetary nebulæ. Referring to Nova Persei, the new star of 1901, in his Presidential Address to the Royal Society in that year, he says: "A remarkable phenomenon occurred in Nova Cygni and in Nova Aurigæ, namely, that at a certain stage of cooling, the bright lines peculiar to the gaseous nebulæ (and which are probably due to an undiscovered light substance we may call nebulum) made their appearance, and, together with the lines of helium and hydrogen, which are common to the nebulæ and early white stars, remained to constitute the latest stage of their spectra."

At the very commencement of his spectroscopic work, the Observatory at Tulse Hill was a meeting place where terrestrial chemistry was brought into direct touch with celestial changes. When the spectra of the elements in 1863 were required in order to determine the position of the stellar lines with accuracy, the necessary observations were made by Huggins. In 1870 the spectra of erbia and other earths were examined by him. In 1880 the flame spectrum of hydrogen burning in air was photographed and mapped. Huggins tells us that in the early sixties his observatory had very much the appearance of a chemical and physical laboratory. Throughout his life he excelled in the laboratory experiments which interpret astronomical observations. In 1897 he published a paper "On the Relative Behaviour of the H and K Lines of the Spectrum of Calcium." He wished to elucidate a problem of great interest—why, although in the general spectrum of the Sun there are no fewer than 70 lines attributed to the element calcium, there are only two—the lines H and K—in the spectrum of the chromosphere and in the spectra of some stars. His conclusions were that this was simply due to differences in the density of the calcium in the source from which the light originated. In 1903 he published in the 'Astrophysical Journal' the results of experiments on the modifications in the appearance of the magnesium line λ 4481 under different laboratory conditions of spark discharge, with a view to the interpretation of its appearance in stellar spectra. Still later, when over 80 years of age, he pursued very delicate investigations on the spontaneous luminosity of radium, showing that it was due to phosphorescence of the nitrogen of the air.

The conclusions at which Huggins arrived on the subject of stellar evolution are given in 'An Atlas of Representative Stellar Spectra,' published in 1899. Lady Huggins is associated with Sir William in the authorship of this beautiful volume, and she has enriched it with initial letters and other drawings, which recall the decoration of the works of the old astronomers. A short history is given of the pioneer researches carried out at Tulse Hill,

and a description of the various instruments employed. These have been already referred to, with exception of the new ultra-violet spectroscope constructed in 1896. The small spectroscope with its slit in the principal focus of the 18-inch speculum was dismounted and the convex speculum was replaced, restoring the original Cassegrain telescope. The new spectroscope was mounted with its collimator passing through the hole in the large speculum to such a distance that the slit was at the focus of the Cassegrain. The spectroscope consisted of two 60° prisms of Iceland spar, the smaller having a length of $2\frac{1}{2}$ inches and height of $1\frac{1}{2}$ inches. The collimator and camera lenses were of quartz, the latter having a focal length of 15 inches.

The 'Atlas' consists of a series of plates reproduced from spectra photographed with this instrument, extending from λ 4870 to λ 3300. They are of special value as giving many typical spectra over a very long range of wave-lengths. Very little work has been done elsewhere on the ultra-violet spectra of the stars. The text contains a discussion of the "Evolutional Order of the Stars and the Interpretation of their Spectra." In 1879 Huggins had selected as a natural criterion, indicating successive changes of density and temperature, the gradual increase of strength of the calcium line K, taken together with the diminution in strength of the lines of hydrogen and the simultaneous incoming and strengthening of the metallic lines. He thus obtains an order, essentially the same as Vogel's of date 1874, and regards the white stars, such as Sirius, to represent the early adult and persistent stage of stellar life, stars like the Sun and Capella, the condition of maturity and commencing age, while in the orange and red stars, such as Aldebaran and Betelgeux, he saw the setting in and advance of old age. This classification is, in the main, adhered to in 1899. An important addition is the separation of the helium stars from other white stars. From their association with nebulæ they are naturally placed first in the order of evolution. From them the change is continuous to the Sirian stars, from the Sirian to the Solar, and from the Solar to the Red stars.

Sir William and Lady Huggins attach more importance to density than to temperature in causing spectral changes, and they point out how large a part is played by the increase of gravity at the surface which accompanies the contraction of a star. They determine the relative temperature of the stars by the comparative intensities of the ultra-violet and blue portions of their continuous spectra. These delicate observations led to the conclusion that temperatures of stars increase till they have reached the stage of the Sun, and then decrease. Here they are at variance with the conclusions favoured by other astronomers, which make the white stars hotter than those of solar types.

The 'Atlas of Representative Spectra' forms vol. 1 of the 'Publications of the Tulse Hill Observatory.' It was followed in 1909 by vol. 2, containing a collection of Huggins' scientific papers, edited by Sir W. and Lady Huggins, reprinted from the 'Transactions' and 'Proceedings' of the Royal Society and other scientific journals. The Address to the British Association at

Nottingham and the Presidential Address at Cardiff, are also included. The volume contains a reproduction of the portrait of Huggins, painted by the Hon. John Collier during the term of his Presidency, which hangs in the rooms of the Royal Society; it has also a photograph of Lady Huggins. The papers are arranged according to the subjects with which they deal; they present a contemporary record of the growth of Astrophysics in its different branches. In the preface Sir William Huggins says: "Looking back, with the knowledge of the more efficient and perfectly adapted instruments and methods of work which have been gradually introduced during the last forty years, no one can be more conscious than I am of the inevitable shortcomings of my pioneer instruments and methods of work which had to be created under circumstances of no little difficulty. These shortcomings prevented the attainment of accurate results in some single cases, but time has shown that they did not affect the fundamental general correctness of my early work." Huggins witnessed a great advance in Astrophysics. But he saw no departure from the methods he employed. The development was due mainly to an increase in light-grasping power, brought about by large telescopes and sensitive dry photographic plates. Huggins realised clearly what problems could be solved by prismatic analysis, and he showed the right way to set to work. His attack combined a splendid audacity with great judgment. This collection of his papers rounded off the main work of his long scientific life. He relinquished in 1908 the charge of the telescopes and spectroscope which had been placed in his care by the Royal Society in 1871. These were given by the Royal Society on his advice to the Astrophysical Department of the University of Cambridge, and were erected there during his lifetime. Arrangements had been made, at the time of his death, for him to visit Cambridge, to inaugurate the completion of this installation.

Sir William Huggins received many marks of distinction in recognition of his scientific achievements. He was elected a Fellow of the Royal Society in 1865, and received a Royal Medal in the following year. The Rumford Medal was awarded to him, most appropriately, in 1880, and the Copley Medal, the crowning honour at the disposal of the Society, in 1898. The Gold Medal of the Royal Astronomical Society was awarded to him along with Prof. Miller in 1867, and to him alone a second time in 1885. In 1869 he was Rede Lecturer at Cambridge, and he received the honorary degree of LL.D. from that University in 1870. Oxford conferred on him the degree of D.C.L. in 1871, Edinburgh the LL.D. in 1871, Dublin the LL.D. in 1886, and St. Andrews the LL.D. in 1893, and he received degrees from various Foreign and Colonial Universities, including Leyden and Heidelberg. The Paris Academy of Sciences awarded him the Lalande Prize in 1872, and elected him a Corresponding Member in 1874; he also received the Valz Prize in 1883, and the Janssen Gold Medal in 1888. In 1901 he received the Henry Draper Gold Medal from the National Academy of Sciences of Washington.

He was enrolled as honorary or foreign member of most of the principal

national learned societies, including the Institute of France, Reale Accademia dei Lincei, the Royal Academies of Sciences of Berlin and Göttingen, the Royal Society of Sweden, the Royal Society of Denmark, the Royal Society of Holland, the (American) National Academy of Sciences, the American Philosophical Society of Philadelphia, and the American Academy of Arts and Sciences of Boston, the Royal Irish Academy and the Royal Society of Edinburgh.

He was created K.C.B. in 1887, on the occasion of the Diamond Jubilee of Queen Victoria. In 1902, when the Order of Merit was instituted by King Edward, Sir William was chosen as one of the twelve who originally constituted the Order.

Sir William Huggins served repeatedly on the Council of the Royal Society. He was three times Vice-President, and in 1900 was chosen to succeed Lord Lister as President. He filled this very important office for five years with marked dignity and distinction. At the suggestion of his colleagues, selections of four of his Presidential Addresses, which treat of subjects of general interest, were published by him in book form. In these he discusses the value of science in education as compared with humanistic studies, considering them as equally essential, and indeed complementary of each other. He also gives an account of the great work that science, as represented by the Royal Society, has done and is doing for the nation.

In 1891 he was President of the British Association at Cardiff.

He served on the Council of the Royal Astronomical Society continuously from 1864 to 1910. He was Secretary from 1867 to 1870, Vice-President from 1870 to 1873, President from 1876 to 1878, and Foreign Secretary from 1873 to 1875 and from 1883 to 1910.

Sir William Huggins gave ungrudging service in all these capacities. Only a week before his death he took part in a meeting of a joint committee of the Royal and Royal Astronomical Societies, for making arrangements for the publication of a collected edition of Sir William Herschel's papers, an undertaking due largely to his initiative. His scientific eminence naturally brought him a great deal of correspondence, to which he gave generously of his time and thought. His unfailing good health enabled him to do this in the midst of his own researches. In the last year of his life he worked for some hours daily in the physical laboratory, spent the afternoon in the study, and wrote in the evening.

His death, at the age of 86, took place in a nursing home in London on May 12, 1910, unexpectedly, after one day's illness.

F. W. D.

GEORGE JOHNSTONE STONEY, 1826—1911.

THE family from which George Johnstone Stoney was derived, on his father's side, settled in Ireland in the seventeenth century, coming from Yorkshire. From the marriage of George Stoney of Oakley Park, King's County, with Anne, daughter of Bindon Blood, D.L., of Cranagher and Rockforest, County Clare, was born in February, 1826, George Johnstone Stoney, eldest son and third child. One other son also was born, Bindon Blood Stoney. The latter, a distinguished engineer, and a Fellow of the Royal Society, died early in 1909. A sister of the late George Johnstone Stoney married her cousin the Rev. William FitzGerald, Bishop of Killaloe, a union which gave rise to the late George Francis FitzGerald, whose remarkable genius, especially in physical science, is known to all. Other distinguished relatives are to be found in William Bindon Blood (mother's brother), who was a professor of engineering and author of professional papers; in General Sir Bindon Blood, G.C.B. (son of mother's brother), Commander of the Forces in the Punjab and distinguished in the Chitral Expedition and in the Boer War; and in Maurice FitzGerald, lately Professor of Civil Engineering in Queen's University, Belfast.

The Stoneys' country property in Ireland was of considerable value during the early years of the last century. Those were times of large profits to agricultural undertakings, the Napoleonic Wars conferring an artificial value on home produce. Irish property fell in value when the wars ceased, and country gentlemen found that encumbrances incurred during the more prosperous times, and as the result of lavish hospitality, were not so easily met as in the good times. Poverty fell upon them and the terrible times of the Irish Famine (1846—48), intensified by the monstrous policy of that day which decreed the local raising of the Poor Law rate just where the famine was most severe, completed the ruin of many Irish families in those districts where the unfortunate tenants stood most in need of the landlord's assistance. The Stoneys' property had to be sold; it fetched about eight years' purchase of the reduced rental, and Johnstone Stoney's widowed mother and her children had no other means.

Many county families who had similarly lost their landed property flocked to Dublin and turned to professional careers in order to make their way in the world. It was a strenuous time in this younger society of Dublin, and one of much mutual helpfulness. Johnstone Stoney and his brother Bindon entered Trinity College, earning the expense of their fees by "grinding," or in English phraseology "coaching." Although unable to afford such assistance for themselves, both brothers had distinguished college careers, Johnstone never failing to obtain a place amongst the first three in the First Class honour lists, and taking a Second Senior Moderatorship in Mathematics and Physics in the Final, the first place going to the

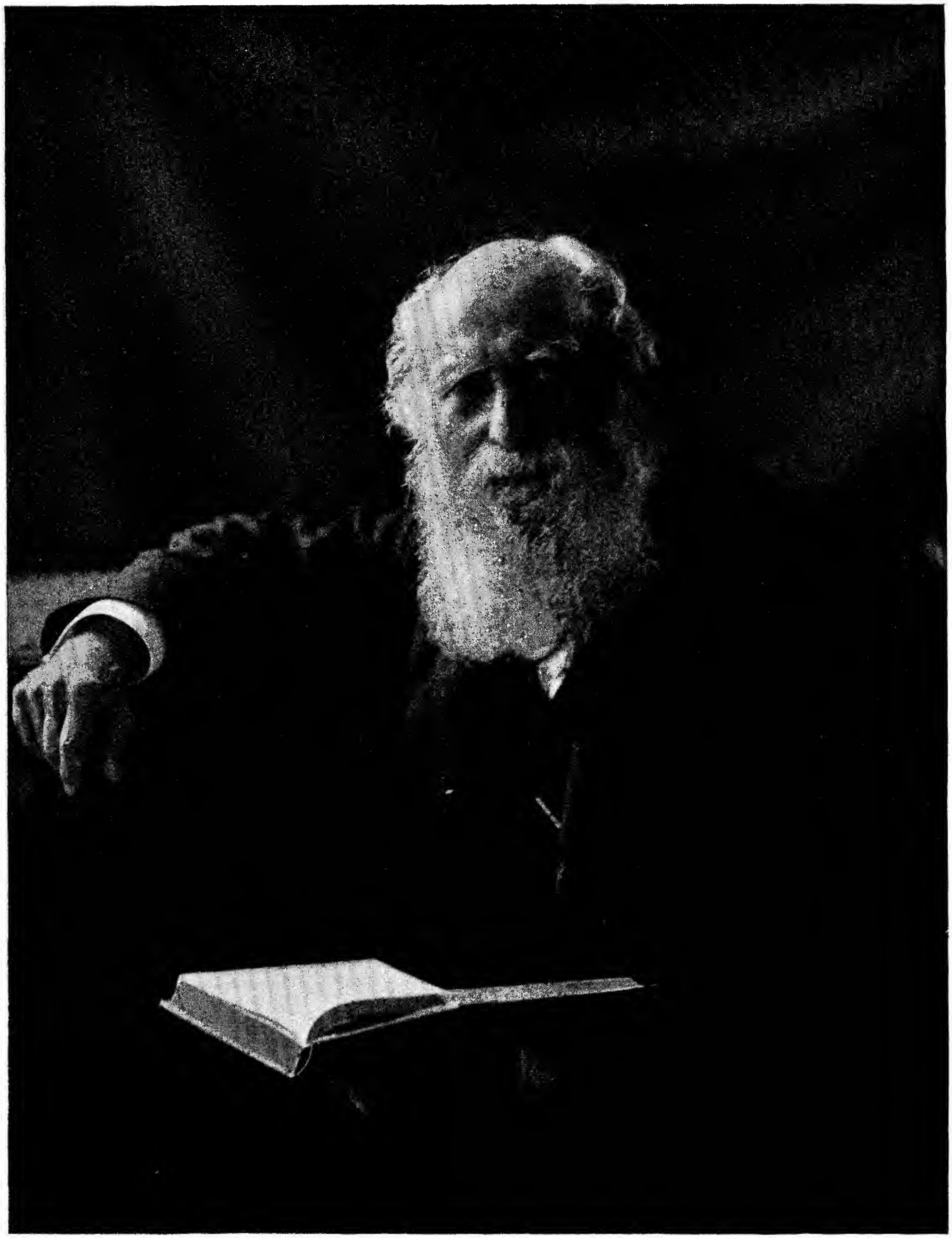

Photo by Miss Edis, 1910. Grout Sc. et Imp.

G. Johnstone Stoney.

distinguished mathematician Mr. Morgan Crofton, F.R.S. In those days scholarships all went to classical men.

When Johnstone Stoney left college Lord Rosse made him his first regular astronomical assistant at Parsonstown, a post subsequently filled by Robert Ball and by other young men who later on made a distinguished mark in science. Stoney read for the Trinity College Fellowship while at Parsonstown. He entered for it in 1852 and took second place, winning thereby the Madden Prize, which is worth about £300. It was the last examination conducted in Latin, and a wide range of subjects was included in the course, the cumulative marks deciding. Although not so severe an examination as it has since become, the ordeal was a severe one. Stoney was examined in Hebrew, chronology, metaphysics, and classics, besides his own special subjects of mathematics and physics. The range of subjects, the method of cumulative marking, and the encumbrance of Latin as a medium of expression, would handicap any science student. It was, too, an examination in which brilliant originality of mind counted practically for nothing. Stoney is one of many distinguished men whom Trinity College has lost as the result of the exclusively examinational mode of entry to Fellowships.

Johnstone Stoney could not afford to try for the Fellowship again, and Lord Rosse, who was always a true friend to him, used his influence to have him appointed to the Chair of Natural Philosophy in Queen's College, Galway. Stoney remained five years in Galway and then became Secretary to the Queen's University, which brought him back to Dublin and to the wider life of a great University city, a change keenly appreciated by him.

For many years after this Stoney devoted himself enthusiastically to the work of solving the problem of the provincial university, in other words to securing to the local university the same beneficent influence which the greater central universities exert upon those more wealthy students who can afford the cost of residence away from home. The problem was then a new one, for although the older universities have in past centuries gone through times of mere provincial importance, the conditions of their growth and development were different from those which affect a recent institution. The latter has to face at once the rivalry of the older institutions, with their greater prestige, and the facilities of modern modes of travel. In Ireland the whole question is complicated by differences in views as to the mental standpoint of the university, differences founded upon religious principles which seem impervious to argument.

In the midst of these labours, which continued till 1882, when the Queen's University was dissolved, other chances came to Johnstone Stoney, but his devotion to what he hoped would have been his life's work prevented his considering them, though some of these offers would have benefited him pecuniarily and others would have given him greater leisure for research, the latter a condition far outweighing the former in his estimation.

The office of the Queen's University was then situated in Dublin Castle, and Stoney's conversancy with educational matters led to his being

frequently consulted by both political parties. He was frequently brought over to the House of Commons, so as to be at hand to give information to the Chief Secretary of the day when Irish educational matters were in question. He wrote many reports on educational subjects, more especially for Sir Thomas Larcom. When Sir Thomas retired, he was anxious that Stoney should succeed him as Permanent Secretary in Ireland. Lord Mayo, then Lord-Lieutenant, sounded him on the subject; however, Stoney frankly told him he approved of Mr. Gladstone's Irish Church Disestablishment Bill as the healthiest policy for the Church itself. This closed the matter, as Lord Mayo was a Conservative and opposed to Mr. Gladstone's policy.

At the request of the Civil Service Commissioners, Stoney added to his other duties those of Superintendent of the Civil Service Examinations in Ireland. The double duties added much to his already heavy office work, and sorely curtailed what leisure he had for scientific work. In 1882 the dissolution of the Queen's University fell as a crushing blow upon Stoney. "At a stroke of the pen, I beheld the labour of nearly thirty years of my life annulled"—in such words he described the event to the present writer. There is no doubt that the policy of dissolution was a most questionable one. The University was dissolved just when becoming most successful. In its stead an examining University was installed, and the several colleges of the old Queen's University became feeders to this institution, which had the power of conferring degrees on purely examinational tests, degrees bearing the same status as those conferred upon students who were not in residence in the colleges. The evils arising, and developing with the lapse of years, in the new Royal University found little defence when, many years later, the latter became the subject of enquiry by the Irish University Commission of 1901. The Commissioners reported that, in addition to defects of organisation, the Royal University "has seriously impaired the value of the University education which was previously in existence. On this side its influence has been one of positive destruction; since it came into being, the growth of the Queen's Colleges has been arrested." In the end, the Royal University, which had supplanted the Queen's, was in turn swept away, and, the present National University of Ireland and the Queen's University of Belfast installed in its place, the purely examinational system being, it is to be hoped, banished for good.

It was during this long period of residence in Dublin that Stoney's influence made itself felt in the policy of the Royal Dublin Society. This Society is unique among voluntary institutions in the United Kingdom—unique in organisation, and, it may be said, unique in the influence it has exerted upon almost every factor tending towards advance of civilisation and national prosperity. Originating in the efforts of a few enthusiasts to better the arts and industries of Ireland, it was founded in 1731, having its first home in Trinity College. Developing year by year, it fostered not only industries but science, and to its inception are due the Royal Botanic Gardens of Dublin, the Royal College of Science, the National Library, the National Museums

of Art, Archæology, and Natural Science, and the National Art Gallery of Ireland.

In this Society Stoney served as Honorary Secretary for over twenty years, and afterwards as Vice-President, until he left Dublin for London, on his retirement from official life, in 1893. During his tenure of office the Society went through great and fundamental changes, exacting much extra work from its officers. The Society used to be the channel through which the Government administered grants in Ireland to Agriculture, Science, and Art. The time came when the Treasury felt that it was anomalous for public grants to be administered by a private, voluntary society and out of the direct control of a Public Department. The Royal Dublin Society, in Stoney's time of office as Honorary Secretary, handed over its great collections to the Government, receiving an allotment of capital for the pursuance of its scientific functions, and for enlarging and amplifying its agricultural shows and otherwise helping Irish agriculture. The premises at Ball's Bridge were acquired at this time, and an era of advance in the magnitude and influence of its shows initiated, which has resulted in making them of international importance.

Towards the close of his personal influence in the Royal Dublin Society, Stoney induced the Council to inaugurate concerts directed to the performance of the best chamber music by proficients brought from various parts of the world; these concerts soon became a permanent part of the Society's work, attracting many members to the Society, and undoubtedly doing much for the advance of musical culture in Dublin.

It would be impossible here to estimate adequately Stoney's influence upon the Royal Dublin Society. The work of reorganisation arising in its constitutional changes was enormous. One who, like the writer, served in the secretaryship during more settled times, can realise what it must have been. The Council is a complex body, sitting together as a Council, working apart as three great Committees of Agriculture, of Science and its Industrial Applications, of General Purposes. There are two secretaryships, which generally are regarded as respectively apportioned to Agriculture and to Science. The latter was filled by Stoney, but the secretaries' work is by no means limited by this subdivision. They enter alike into all the questions of public policy continually arising, and into the Financial Committee's supervision of ways and means.

Additional to his general work for the Society Stoney worked wholeheartedly for the advancement of its scientific functions. For many years his own research work was communicated almost exclusively to the Society, and published in its 'Proceedings' and 'Transactions': his aim being to confer upon its publications something more than a local prestige. His gifted relative, George Francis FitzGerald, ably assisted him in this endeavour. The younger men, feeling the advantages of discussions in which men of such critical ability participated, gladly brought their work to the Society, and for many years the evening meetings were characterised by debates of the highest scientific interest. Although the Society still

does invaluable work for science, there is no doubt that in the untimely death of FitzGerald, and the loss of Stoney upon his departure to London, the science meetings lost much of their high standing.

The close of his official work in Dublin meant for Stoney a time of leisure; but it came too late for the publication of much of the scientific work he had done. He was then nearly 68 years of age, with health impaired by the long and heavy strain of official work. His life in London was largely devoted to the completion and publication of original work begun in earlier years; but this could only be slowly accomplished, and he died before it was completed according to his wishes.

Stoney's scientific work needs no apology on the score of diversity or of prolificness; but the amount of it conveys but a small idea of his life's work, and, indeed, forms but a small part of it. Only an over-mastering scientific enthusiasm could have elicited such a body of work from a man harassed for the best years of his life by strenuous duties continually leading his thoughts into other channels. Yet a major part of his work was accomplished during those years of official toil. He was a remarkable instance of the resistless power of a great intellectual development; of its relentless pertinacity and resolution. His power of accomplishment was linked with an unusual degree of self-centred devotedness to the immediate subject of his thoughts and speculations. Around this subject the interests of his intellectual life appeared to be gathered and concentrated for the time. With this very conspicuous quality of mind it is the more remarkable that he worked as a devoted and successful official during years of considerable scientific fertility. It must have been a heavy exaction even from one with his high-minded sense of duty. The final facts of his success in both spheres of his work demonstrate at once his intellectual force and his splendid devotion to his aims and to his duties.

One of Stoney's earliest papers was a geometrical examination of the conditions of propagation of undulations of plane waves in media ('Trans. Roy. Irish Acad.,' vol. 24, 1861). The reasoning is mainly directed to explaining why an undulation of the kind considered, when once established, continues to propagate itself in one direction only. Analytical reasoning is not used. Indeed, throughout a major part of his writings Stoney prefers geometrical to analytical reasoning. In a later paper he states his preference for the former: "The chief value of the geometrical form of proof is that it gives us a more continuous view of what is going on in nature, inasmuch as the stages of the geometrical proof of a physical problem keep throughout their whole progress in close proximity to what actually takes place, whereas a symbolical proof is in contact with nature only at its commencement and at its close" ("On a New Theorem in Wave Propagation," 'Phil. Mag.,' April, 1897).

After the appearance of his optical paper of 1861 the subject of geometrical optics does not appear to have enjoyed Stoney's attention till the appearance of the "Monograph on Microscopic Vision" ('Phil. Mag.,' October, November,

and December, 1896), that is, till after his official life was closed and he had leisure to work up material which, as in this case, had probably been by him for many years. The study of microscopic vision is based upon a method of resolution into flat wavelets. Stoney shows that the method is one of wide generality, and a series of propositions occupy a large part of the paper, establishing and analysing the fundamental proposition that "however complex the contents of the objective field . . . the light which emanates from it may be resolved into undulations, each of which consists of uniform plane waves," or wavelets, which do not undergo change as they advance. On this basis the causes of the phenomena presented by microscopic vision are sought in a paper ingenious in reasoning and laborious in its scope. The proof of the fundamental proposition given in the first paper did not, however, satisfy Stoney, and several subsequent papers appeared, one criticising an insecure proof advanced by Thomas Preston, "On a Supposed Proof of a Theorem in Wave-Motion" ('Phil. Mag.,' May, 1897); also, on this subject, see 'Phil. Mag.' for February, April, July, and August, 1897, and July, 1898. A proof of the theorem by the principle of reversal is given in a paper published at p. 570 of the 'Report of the British Association' for 1901; and Stoney returns to the matter again in the 'Philosophical Magazine' of February, 1903, this time partly with a view to welcome an analytical proof of the resolution into flat wavelets by E. T. Whittaker. This paper, entitled "How to Apply the Resolution of Light into Uniform Undulations of Flat Wavelets to the Investigation of Optical Phenomena," contains several theorems not contained in earlier papers. All this work was written in ignorance of the fact that Stokes, in one of his earlier papers (1845), had enunciated the same fundamental proposition from which Stoney's work takes origin—but without offering any proof. In a paper contributed to the 'Philosophical Magazine' in April, 1905 ("Flat-Wavelet Resolution, Part III"), Johnstone Stoney announces his discovery of Stokes' priority. It is probable that Sir George Stokes considered it almost self-evident as a general statement " . . . for we may represent an arbitrary disturbance in the medium as the aggregate of series of plane waves propagated in all directions." In an appendix to this paper of April, 1905, Stoney again considers the proof of the theorem, and from a manuscript note inserted in his "Monograph on Microscopic Vision" it would appear that the last mode of regarding the matter was that which he preferred.

Stoney's last published scientific papers were on telescopic vision ('Phil. Mag.,' August, November, and December, 1908), and these are now referred to because they are a continuation of the subject to which Stoney directed his attention early in life—consideration of wave-propagation and the formation of images. In treating of telescopic vision Stoney resolves the light before it enters the telescope into a somewhat special system of undulations of spherical wavelets, *i.e.* into spherical undulations, the centres of which shall be the several points of a plane perpendicular to the optic axis and situated close in front of the objective, and by the interference of

which (in the usual manner), at the principal focus, the image is formed; the papers are of the same character—at once ingenious and laborious—as that upon microscopic vision. It is a surprising reflection that Stoney was in his eighty-third year when these elaborate and painstaking papers were penned.

The papers alluded to above represent important work, and are characteristic of Stoney's manner of dealing with investigations of similar description. Their inception dates from the beginning of his scientific career, and they have, therefore, been placed in the forefront of this brief review of his work, but they are by no means the most important part of his work. His investigations in various departments of molecular physics distinctly claim that place.

Stoney's work in molecular physics began in 1860, when, on the basis of Maxwell's estimate of the average length of the free path of the molecules of a gas, he made an estimate of the number of molecules present in unit volume. This was, of course, led up to also by the work of Clausius, and was an early contribution to a great subject, then only beginning to be a subject of research. Waterston's memoir had been submitted fifteen years earlier (1845), and, not being published, the advent of the new ideas had to wait for Clausius' papers of the later years of the forties. For the first time the kinetic theory then received publication, and it was recognised that planetary conditions as regards freedom of motion might attend the movement of a gaseous atom between its encounters. Stoney showed a clear and vivid appreciation of the new molecular science, and from this time forward the application and exposition of its laws occupied him at intervals throughout his life. We gather some idea of the state of the subject by perusing a paper by Stoney appearing in the 'Proc. Roy. Irish Acad.,' vol. 7, 1858. This appears to be his earliest contribution to the subject. We find him demonstrating that the law of Boyle is contrary to the view that the particles of a gas are at rest, or that it can be a continuous homogeneous substance.

Ten years later, writing in the 'Philosophical Magazine' "On the Internal Motions of Gases Compared with the Motions of Waves of Light" ('Phil. Mag.,' August, 1868), we find him complaining that the dynamical theory of gases had not met with the general attention and acceptance which it deserved. In this latter paper a vivid appreciation of the relative magnitudes is shown, and Stoney pictures the source of the light waves as existing in the "from fifty to one hundred thousand of these little orbital revolutions" which the molecules are able to execute between successive encounters. Such thoughts were more fully elaborated later. He closes his review of the subject with his estimate of molecular numbers in a gas at standard pressure and temperature, concluding that in 1 cubic mm. there are 10^{18} molecules.

Arising out of his interest in the kinetic theory of gases are his series of papers on the conditions limiting planetary atmospheres. He first touches

on the subject in his paper "On the Physical Constitution of the Sun and Stars," which appeared in the 'Proceedings' of the Royal Society in 1868. In this he infers that a complex atmosphere will, near its outward quiescent boundaries, cease to be homogeneous, the lighter constituents extending further into space. Towards the close of 1870, in a discourse delivered before the Royal Dublin Society, the subject of the absence of atmosphere from the moon is discussed, the conclusion being that the gravitation on the moon will not suffice to retain a free molecule moving in a radial, or even outward, direction, with a velocity of 2·38 kilometres per second. Molecules which occasionally reach this speed may be, accordingly, lost to the moon. A full account of his views is given in his paper "On Atmospheres of Planets and Satellites" ('Trans. Roy. Soc. Dub.,' vol. 6, 1897). Stoney contends that on the earth hydrogen and helium are scarce or absent because of their leakage from the atmosphere; water molecules, on the other hand, cannot attain so great an excess over the velocity of mean square as is required for their escape. The theory has been called in question on deductive reasoning (see papers by S. R. Cook, 'Astrophys. Journ.,' vol. 11, Jan., 1900: and by G. H. Bryan, 'Phil. Trans.,' A, vol. 196, March, 1900). Stoney's argument rests, as he admits, on inductive reasoning, based on the observed facts of the absence of atmosphere from the moon, and the scarcity of helium and hydrogen on the earth, and he meets the deductive objections by questioning the adequacy of the mathematical theory to include all the events which may lead to excessive velocities of isolated molecules in the upper atmosphere. The discussion is too long to enter upon here. In the limit we must accept as true that a small asteroid could not retain an atmosphere by its gravitational attraction; and the view, held by some, of an asteroidal origin of our earth would appear to meet considerable difficulty here, more especially with regard to the existence of the terrestrial hydrosphere. There is no doubt that Stoney's theory removes difficulties in explaining the absence of a lunar atmosphere. It has been applied, too, to the condition apparently obtaining on Mars. However, it must be admitted that other causes may exist to account for the condition obtaining in those bodies.

The discovery by Crookes that a blackened vane suspended in a high vacuum is repelled by radiant heat or by light led to various suggestions as to the cause of the phenomenon. Stoney offered an explanation in harmony with the various experimental conditions which have to be fulfilled in order for the Crookes force to be developed. The theory of Stoney is given, in a somewhat crude state, in the 'Phil. Mag.' for March and April, 1876. Stoney's view may be summarised in the statement that for a certain distance in front of the heated vane, and reaching from it to the glass envelope, when the vessel is not too large, the molecular motions of the rarefied gas are polarised by the thermal conditions, and interpenetrate one another in a degree greater than prevails elsewhere in the gas. The greater molecular interpenetration in the line

between glass and vane involves nothing of the nature of a wind, but determines a greater stress in the direction of polarisation. Errors in the earlier statements of his views are corrected in his paper in the 'Trans. Roy. Soc. Dub.,' 1878, and republished in the 'Phil. Mag.' of December, 1878; and a mathematical expression for the Crookes stress is given, based upon an investigation of Clausius of the stress across a layer of gas conducting heat normal to a heater and cooler. FitzGerald took a part in the discussion (*cf.* his 'Collected Papers') by showing that the stress parallel to the heater and cooler could not be the same as the perpendicular stress—or, in other words, a polarisation stress must exist ('Nature,' vol. 17, p. 514)—and by a mathematical discussion of the subject in the 'Trans. Roy. Soc. Dub.,' 1878.

In 1874, in a paper 'On the Physical Units of Nature,' read before the Belfast meeting of the British Association, Stoney pointed out that, on the basis of Faraday's law of electrolysis, an absolute unit of quantity of electricity exists in that amount of it which attends each chemical bond or valency. This paper was printed afterwards in the 'Phil. Mag.' for May, 1882. He suggests that this might be made the unit quantity of electricity. He subsequently suggested the name *electron* for this small quantity. Von Helmholtz, in 1881, independently, drew attention to the existence of such definite elementary charges, which behave like atoms of electricity. Stoney estimated the magnitude of the electron in 1874, finding it to be equal to the unit (then the ampère) $\times 10^{-20}$. This is the same as 1 C.G.S. electrostatic unit $\times 3 \times 10^{-11}$. In this estimate he avails himself of his determination of the number of molecules present in 1 cubic mm. of a gas at standard temperature and pressure, viz., 10^{18}. That the result should suffer from the errors in the then available data detracts nothing from the merit of Stoney's performance. It was pioneer work in an obscure and difficult line of research.

The conception of one or more unit charges of electricity within the atom was soon applied by Stoney to the phenomena of spectral dispersion. Maxwell's ideas on the electromagnetic nature of light were first published in 1862 and 1866, but the final statement of his theory only appeared with his great work on 'Electricity and Magnetism' in 1873. In Stoney's first paper, which deals with the "Internal Motions of Gases compared with the Motions of Waves of Light," which appeared in the 'Philosophical Magazine' for August, 1868, no reference to Maxwell's views is made, nor is there, of course, any suggestion of the electronic origin of light waves. The aim of the paper is to point out that there must be periodic motions within the "molecule" to occasion the spectral lines, motions distinct from those translatory ones which are affected by the temperature of the gas. The latter are irregular, the former are in general regular, save at the instant of collision—an instant short in comparison with the time occupied in describing the mean free path. The causes of continuous and band spectra are referred to. The nature of the internal motions must differ in different gases. A

further step is taken in his paper of January, 1871 ('Proc. Roy. Irish Acad.'). The internal atomic motion is a complex periodic motion which, however, is resolvable into harmonics. If we assume the undulation arising in the ether to consist of periodic plane waves, then, whatever its form, it may be regarded as formed by the superposition of simple pendulum vibrations, one of which has the full periodic time, while the others are harmonics of this vibration, which may be developed by Fourier's theorem. While these component vibrations are superimposed in free ether, on entering a dispersive medium the several vibrations no longer keep together, and a physical resolution is effected in the spectrum. Hence simply harmonic sequences of spectral lines would arise from the distinct motions in the molecule of the gas, for there may be several such motions, each producing its own series of harmonics. Applying these views to the case of the ordinary hydrogen spectrum, Stoney finds that the lines *h*, F, and C are nearly the 32nd, 27th, and 20th harmonics of a fundamental vibration whose wave-length *in vacuo* is 0·13127714 of a millimetre, this agreeing closely with Ångström's measurements. A few months later, Stoney, in conjunction with Emerson Reynolds, advances yet further, finding a serial relationship, of the kind referred to above, in a large number of lines in the absorption spectrum of chlorochromic anhydride. But the general result as to the existence of simple harmonic relations was challenged by Schuster and others, on the ground of the theory of probabilities, the instances being held to be too few to establish a case. It was some years later that the observations of Huggins upon stellar spectra led to an extension of the hydrogen spectrum, as this had been observed in solar light; and in 1885 Balmer showed that a comprehensive law for the whole system of hydrogen lines was expressible in a single formula of quite different type; and a train of ideas was thus introduced, which has led to much subsequent work directed to the sorting out of related series in the lines of a spectrum.

Stoney, in his principal paper on this subject ('Trans. Roy. Soc. Dub.,' vol. 4, May, 1891), states his electronic theory of the origin of the complex ether vibrations which proceed from a molecule emitting light. The paper is "On the Cause of Double Lines and of Equidistant Satellites in the Spectra of Gases." His theory is based on the electromagnetic theory of light, and refers a series of spectral lines to the periodic motion of an electron in the atom or molecule, the elliptic partials into which this motion may be resolved by Fourier's theorem accounting for the several lines. If perturbing forces exist an apsidal motion may affect the elliptic partials, and Stoney shows that, while the undisturbed orbit will in general be such as to give rise to a definite series of single lines in the spectrum, the consequences of an apsidal motion affecting some, or all, of its partials is to cause the corresponding lines of the series to become double. He deduces, on these views, the result that the double D lines of sodium in the solar spectrum might be accounted for by the motion in each molecule of an electron in an elliptic orbit having an axial ratio lying between 11 to 1 and 13 to 1, round which

ellipse the electron revolves 169,637 times in a "jot" of time, the ellipse being slowly shifted round with an apsidal motion which carries it once round while the electron performs 1984 revolutions. Similarly, precessional motion will occasion triple lines. The "jot" of time is the time light takes to traverse one-tenth of a millimetre *in vacuo*.

A very large amount of work has been done by mathematical physicists within recent years on theories of atomic structures involving the electron in motion; and, again, the importance of the electron in views on the phenomena of the vacuum tube, and on radioactivity, is known to all. Atomistic ideas as to the nature of electricity were, of course, held before Stoney's views were expressed, but there was a period when the continuous theory had largely displaced the atomistic view. This seems to have arisen mainly from Maxwell's teaching. The more recent and, it must now be admitted, more helpful atomistic theory, in its modern development, dates back to the finding of the electron in Faraday's law of electrolysis by Stoney and Helmholtz; and Stoney's use of the electron in a light-giving atom is one of the earliest developments, showing the availability of the conception of a small discrete particle of electricity. This, in the present writer's opinion, is Stoney's most important work for science.

It may be that a very different conception of intra-atomic structure will ultimately prevail, but the moving electron as a constituent part has not as yet found a good substitute. The phenomena of radioactivity have strongly confirmed it. The early work of Thomas Preston on the Zeeman effect also confirms it. Such recent views as those of Ritz, on atomic structure and the explanation of the Zeeman phenomena, assume, indeed, other sources of action and reaction within the atom, but the electron still remains as generator of electromagnetic waves. And even if the electron ultimately yields place to new conceptions it has helped to forward investigation in many lines of research, and those who first gave it to theoretical science have taken a worthy part in the advance of man's knowledge of Nature. The early date of Stoney's work and the clearness and the fullness with which he urged his views certainly entitle him to a leading place among those pioneers.

Stoney gave much time and thought to the subject of the units of physical science and their nomenclature. He served upon the Committee of the British Association for the selection and nomenclature of dynamical and electrical units in 1873—a committee whose recommendations have been very generally accepted. His paper "On the Physical Units of Nature," which was read before the Belfast meeting of 1874, has already been referred to. In it he urges the claims of "the single definite quantity of electricity" observed in electrolysis as a unit of electrical quantity. The paper is printed in the 'Proceedings' of the Royal Dublin Society, 1881. Several other papers relating to the subject of units came from his pen, and throughout his many papers bearing on other subjects he frequently suggests new departures in nomenclature. Indeed, it may be said that his desire for the perfection

of brevity and reasonableness introduces some difficulties in the study of his papers, seeing that in some cases an unwonted nomenclature has to be first acquired. His services to the subject of physical mensuration have, however, been great; and till quite late in his life he laboured to facilitate the introduction of the metric system into this country.

The circumstances of Stoney's early life led, as has been mentioned, to his appointment as observer to Lord Rosse at Parsonstown. The interest in astronomy then aroused remained with him throughout life. He wrote both on the instrumental equipment of observatories and on the objects of the heavens. Thus there are papers "On Collimators for Adjusting Newtonian Telescopes" ('B. A.,' 1869); "On the Equipment of the Astrophysical Observatory of the Future" ('Monthly Notices,' 1896); "On the Mounting of the Specula of Reflecting Telescopes" ('Proc. Roy. Soc. Dub.,' 1894). His other papers are principally upon the Leonids. In one of them, a discourse before the Royal Institution (1879), the idea of comets capturing meteorites in virtue of the retardation experienced by the latter when passing through the gaseous substance of the comet is put forward (see also 'Monthly Notices,' June, 1867). Other papers are upon the physics of the solar atmosphere; one of them has already been referred to, another was published in the 'Philosophical Magazine' of December, 1868.

In 1888 Stoney entered upon a study of the numerical relations of the atomic weights. An outline of his results appears in the 'Proceedings' of the Royal Society, April, 1888. The full paper has not been published. The leading idea is that if a succession of spheres be taken whose volumes are proportional to the atomic weights ("atomic spheres"), and the radii of these spheres are plotted on a diagram as ordinates, and a series of integers as abscissæ, a logarithmic curve, $y = \mathrm{K} \log(qx)$, is developed which, in the belief of the investigator, shows that the atomic weights follow laws which can be represented as the intersection of two definite mathematical curves; implying that two definite laws of nature have to be coincidently fulfilled for an atom to come into existence. The curve so represented passes nearly through the positions given by observations. The discussion as to how to reconcile the curve with the slight perturbations, and why neighbouring logarithmic curves pursuing courses close to the observed positions are excluded, occupies several sections of the paper. He also gives a polar diagram in which the radii of the atomic spheres are used as radii vectores. This diagram suggested to him, in the first instance, the logarithmic spiral. The diagram is of much interest, and finds publication in the 'Phil. Mag.,' September, 1902. The quadrants of the figure are alternately found to include electro-positive and electro-negative elements. An unoccupied sesqui-radius appears in the diagram at a place where alone an *abrupt* transition from the electro-positive to the electro-negative character is observed. The inert gases discovered some years later now occupy this radius. In the 'Phil. Mag.' of September, 1902, Stoney suggests that the unusual chemical behaviour of these new elements is a consequence of their

occupying a position between the halogen radius, in which the electro-negative condition attains its greatest intensity, and the radius containing lithium, sodium, potassium, rubidium, and cæsium, which are the most electro-positive of the elements. The prediction of missing elements on the indications of the logarithmic law is notified specially by Stoney in a letter to the 'Phil. Mag.,' October, 1902. He suggests here that the new elements will possess the greatest atomic volumes among the elements in the solid state. The specific gravity in the solid state of these bodies has not as yet been determined.

There is no doubt that the logarithmic curve given by Stoney is suggestive in the highest degree, and is a most interesting contribution to this subject. Stoney had the matter very much at heart, and the non-appearance of his full paper evidently caused him much pain. Stoney believed that a mistaken view was taken of what he really aimed at, this belief being supported by a note of Sir George Gabriel Stokes appearing in 'Stokes' Scientific Correspondence,' vol. 1, p. 219. In the month of March, 1911, Stoney, then upon his death-bed and already worn with many months of illness, dictated a memorandum on the mathematical principles which influenced him in his work upon the logarithmic law of the elements. There is no sign of failing power in this memorandum. Extracts from the original manuscript were in consequence made by Lord Rayleigh, and were communicated by him and published in the 'Proceedings' of the Royal Society (A, vol. 85, p. 471, July, 1911). In these extracts the spiral curve is again reproduced.

A considerable number of scientific subjects, additional to those already referred to, engaged Stoney's attention at various times. They range over a wide field of scientific enquiry and often show much originality. In the 'Phil. Mag.' for April, 1890, he suggests that bacteria may derive a part of their life-energy by relations towards the faster moving molecules in the surrounding medium, of a selective nature, so that they escape the second law of thermodynamics much as the Maxwell demon might have done. A very different topic is "The Magnetic Effect of the Sun or Moon on Instruments at the Earth's Surface" ('Phil. Mag.,' October, 1861); also "On the Energy Expended in Driving a Bicycle," in conjunction with his son, Mr. G. Gerald Stoney, F.R.S. ('Trans. Roy. Dub. Soc.,' 1883); Address to the Mathematical and Physical Section of the British Association, 1879; "On Denudation and Deposition" ('Phil. Mag.,' April and June, 1899), etc.

Johnstone Stoney served on several Committees of the British Association. His name appears in Reports on Solar Radiation, Catalogue of Spectral Rays, on papers connected with Spectrum Analysis, 1881; and he acted as reporter of a lengthy compilation of the Oscillation-frequencies of Solar Rays, 1878.

The subject of Ontology engaged his attention for a considerable time: a paper "On the Relation between Natural Science and Ontology" was

communicated by him to the 'Proceedings' of the Royal Dublin Society in 1890. This paper is in the highest degree characteristic at once of Stoney's mental attitude towards Nature, his methods of logical analysis, and the tendency he so often shows of a desire to build up a subject in its entirety and from first principles, framing for the purpose new words and new definitions. As already remarked, the tendency to revising the ordinary use of language so as to give it more direct significance and more convenient form often imposes some labour upon his readers. The ontology paper is a really profound and exhaustive review of the subject, and indicative of keen introspection, but it is difficult reading on account of the large amount of definition which the writer deems essential. A second part of the essay was published in 1903 by the American Philosophical Society. An earlier allied essay is "On how Thought presents itself among the Phenomena of Nature," being a discourse delivered before the Royal Institution, February, 1885. A few papers on what may be called abstract physics may be mentioned here: "Survey of that Part of the Range of Nature's Operations which Man is Competent to Study" ('Proc. Roy. Soc. Dub.,' 1899); "On Texture in Media and on the Non-existence of Density in the Elemental Ether" ('Proc. Roy. Soc.,' 1890); "Curious Consequences of a well-known Dynamical Law" ('Proc. Roy. Soc. Dub.,' 1887), etc.

Stoney was keenly alive to the charm and refining influence of music, and, as already stated, did much for the study of music under the auspices of the Royal Dublin Society. He wrote a paper "On Musical Shorthand," and in the same volume (1882) of the 'Proceedings' of the Royal Dublin Society is one on methods of dealing with echoes in rooms. In 1883 he suggests, in the same journal, a mode of prolonging the tones of a pianoforte.

Enough has now been said with reference to his scientific work to show how wide in scope it was. Stoney wrote on other subjects, however: "On the Demand for a Catholic University" ('Nineteenth Century,' February, 1902): and in the interests of his University, he writes upon the subject of its reform in 1874, and speaks in its defence against the legislation which threatened it in 1907. As late as 1910 he printed a thoughtful pamphlet on "The Danger which in our Time threatens British Liberty."

No man ever lived more completely and devotedly for his ideas than did Johnstone Stoney. He was the type of the philosopher. Nothing could check his ardour for research; no labour was too great for him to undertake in the pursuit of his ideas. In spite of heavy office-work which afforded none of the long-vacation leisure of university life, in spite of the absence of that stimulus which comes from a professional scientific life, Stoney published two or three papers each year. Through his middle life he rose at five o'clock in order to get in some scientific work before starting for his office. He was never a very strong man, and this necessitated much restraint as to evening society functions. His Sundays were largely devoted

to experiments or writing. His annual holiday was usually for the ten days of vigorous intellectual life of a British Association meeting. At all times he greatly grudged the time and labour of writing down and putting through the press work which had been a pure delight to carry out.

Of the moral attributes of Johnstone Stoney it is impossible to speak without a feeling of profound respect. His fearless love of truth was bound up with an ideal rectitude of life. He stood above all creed that could not appeal to the rationality of man and that denied the continuity of Nature's laws. Intellectually superior to most men, he was yet at once too great and too benevolent to criticise the littleness of the many, the shallowness of their minds, and the fallacies of their tenets. This did not arise in abstraction from the struggle of life and its troubles, for no more sympathetic and kindly man ever breathed. He championed every earnest effort, more especially endeavouring to forward the interests of the younger scientific men with whom he came in contact, in this respect meting to others that same treatment which he himself received at the hands of his early and life-long friend, the Earl of Rosse. Stoney's word was a law to him: what he promised he performed. This is a moral quality which soon gets known, and confers a just influence upon its possessor, not only with those who also possess it, but again with those deficient in it. When Stoney left Dublin, and the occasion was taken by the Royal Dublin Society to present him with a memento of his work for the Society, and again when he received their Boyle Medal, the recognition of his high moral qualities was in the minds of all, and could not be kept out of analyses which were intended to embrace only his social and scientific work.

George Johnstone Stoney received many distinctions during his long and laborious life. Probably the one he most highly valued was the receipt of the first Boyle Medal from the Royal Dublin Society. The medal had just been founded to commemorate the great Irishman who had so large a share in the initiation of the parent scientific society of this country. It was felt by the Council of the Royal Dublin Society that Stoney, above every other Irishman then living, merited the distinction of having his name placed first upon the roll. It was conferred upon him in 1899. He was a Foreign Member of the Academy of Science at Washington, and of the Philosophical Society of America founded by Franklin. He was a corresponding member of Sci. di Lettere ed Arti di Benevento. He was President of Section A at the meeting of the British Association in 1879. He served as Vice-President of the Royal Society under Lord Lister, and also served upon the Council, 1898—1900.

Stoney married his cousin Margaret Stoney. He leaves two sons and three daughters. His eldest son has risen to distinction as an engineer, having been collaborator in the development of the steam turbine with the Hon. Sir Charles Parsons, K.C.B., F.R.S., and is now manager in the Parsons Turbine Works. One daughter is a Lecturer at the London School of Medicine for Women, and another daughter is a London physician. George Johnstone

Stoney died in the eighty-sixth year of his age, on July 5, 1911, after a long illness. His body was cremated, and his ashes buried in the graveyard of the little suburban town of Dundrum, Co. Dublin.

In stature, he was tall; in bearing, dignified; and his features and expression revealed at once his intellectual power, his nobility of character, and his kindly and sympathetic disposition. The portrait prefixed to this memoir was taken in the year 1910, in the eighty-fifth year of his age. It is in every way faithful and excellent.

J. J.

OBITUARY NOTICES

OF

FELLOWS DECEASED.

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GIOVANNI VIRGINIO SCHIAPARELLI, 1835—1910.

Giovanni Virginio Schiaparelli was born at Savigliano, in Piedmont, on March 14, 1835: he graduated at the University of Turin in 1854, and studied the practice of astronomy at Berlin Observatory between the years 1856 and 1859, when Encke was the Director. After spending a short time at Pulkowa under W. and O. Struve, he returned to Italy in 1860 to take up the position of assistant to Carlini in the Royal Brera Observatory of Milan, where he spent the remaining years of his life, succeeding Carlini as Director in 1862. He held this position for thirty-eight years, and was succeeded on his retirement in 1900 by Prof. Celoria.

Schiaparelli early proved his mettle by discovering the minor planet Hesperia in 1861, when such a discovery was still something of an event but his first great work was the recognition that meteors were distributed along definite orbits, and that these orbits coincided with those of known comets. He observed that the meteors which occur in greater or less force annually from August 6 to 12, radiated principally from points in the constellation Perseus, and that those which did so possessed a distinctive character of their own, showing that they belonged to a single family. Such a fact is geometrically sufficient to determine the plane and perihelion of their orbit if the eccentricity or period of revolution is assumed, and this determination showed an agreement which could not be accidental with the orbit of the comet I of 1862.

A more signal instance was supplied in 1866, the year of the great shower which radiates from γ Leonis. This orbit proved to be identical with that of Tempel's Comet II of the same year. For this striking discovery the Paris Academy of Sciences awarded the Lalande Prize to Schiaparelli in 1868, and the Royal Astronomical Society its Gold Medal in 1872.

The Brera Observatory was provided with an 8-inch equatorial telescope by Merz,—the same aperture as that which Dawes had used for his valuable maps of Mars in 1864. A comparison of what we owe to that keen-sighted observer with what Schiaparelli afterwards accomplished with equal optical aid is a searching test and proof of the latter's skill. When he turned his attention to the surface of Mars, he completely transformed our knowledge of the face of that planet. Beginning with the opposition of 1877, he followed it on successive occasions up to 1890, producing more and more detailed maps and masses of observation. The later ones were made with a more powerful telescope, by the same maker, of 19 inches aperture. The whole series of measures and observations are wonderfully voluminous as well as delicate, and they are eminently reliable. It may be said that all that Schiaparelli delineated has been in one way or another confirmed.

On Proctor's map, which was based upon Dawes' observations, a few long dark bands leading from the "seas" were shown and were named by him

"inlets." Schiaparelli added a host of finer ones, making an irregular meshwork over the whole of the "continents." In describing them he translated "inlet" by "canale," and so introduced, apparently by accident, a term whose artificial implications have served as the channel of so much speculation as to the state of Mars. He showed further that the "canals" and other delicate features were not fixed like the features of the moon, but were subject to fluctuations relatively to one another, and independently of the conditions of vision. Sometimes they were strongly marked, sometimes invisible, sometimes narrow and dark, sometimes broad, sometimes doubled.

Schiaparelli always guarded himself carefully from countenancing any theoretical conclusion as to the nature of these markings. In his latest memoir but one he speaks of them as "the lines (or so-called canals)," and refers to an earlier statement where he tentatively attributes the duplication to some unknown atmospheric cause.

Schiaparelli turned his attention to the surface of Mercury in 1882, and to that of Venus in 1890, and came to the conclusion—as to the former planet, one now generally accepted—that each rotated as the moon does about the earth, so that it always turned the same face to the sun. He was also an excellent and very industrious observer of double stars. He published in 1903, after his retirement, a work entitled "L'Astronomia nell' Antico Testamento," in which he showed the same industry and acuteness that he brought to observation, coupled with a singular wealth of learning. It has been translated into English and German.

He was elected a Foreign Member of the Royal Society in 1896. He died on July 4, 1910.

R. A. S.

JACOBUS HENRICUS VAN'T HOFF, 1852—1911.

JACOBUS HENRICUS VAN'T HOFF was born at Rotterdam on August 30, 1852, his father being a physician of that city. After having received his school education in Rotterdam, he entered the Polytechnikum at Delft in 1869, where he completed the ordinary three years' course in technology in two years. In 1871 he entered the University of Leiden, passing the "Kandidatsexamen" of that university in 1872. Attracted by the fame of Kekulé, he went to Bonn in the same year to study organic chemistry. During these "Wanderjahre" he also spent a short time in the laboratory of Wurtz in Paris. Returning to Utrecht to continue synthetical organic work under Mulder, he obtained on December 22, 1874, the degree of "Doktor der Wis-en Natuurkunde," with a research on cyanacetic and malonic acids.

But already in September, 1874, van't Hoff had laid the foundation of his reputation by the publication of a short pamphlet in Dutch, in which he unfolded his views on the extension of structural chemical formulæ to three-dimensional space, and on the relation between optical activity and chemical constitution. The time was ripe for such a development. Pasteur, in 1861, in his classical 'Leçons sur la Dissymétrie Moléculaire,' had shown the connection between optical activity and hemihedral crystalline form, and had perceived that, in the two isomeric optically active forms, the molecules must possess an asymmetric structure similar to that of object and mirror-image. In fact, Pasteur had suggested that the atoms surrounding the carbon atom might possess a tetrahedral arrangement in space. But it was the work of Wislicenus on the lactic acids which chiefly influenced van't Hoff. The fundamental discovery of the latter lay in the recognition of the part played by the so-called "asymmetric carbon atom," that is, a carbon atom united by its four valencies to four different groups or atoms. Van't Hoff showed that optical activity, and the existence of two optically active isomers, differing merely in the sign of their rotations, only occurred when such an asymmetric carbon atom was present. Very similar ideas were published by J. A. Le Bel, quite independently of van't Hoff, two months later, namely, in November, 1874; but van't Hoff discussed the "geometrical" isomerism in the case of unsaturated compounds more fully than Le Bel.

In 1875 he published a much enlarged French edition of his original pamphlet, with the title 'La Chimie dans l'Espace,' whilst a German edition, with a preface by J. Wislicenus, appeared under the title 'Die Lagerung der Atome im Raume' in 1877. Van't Hoff's views were violently attacked by Kolbe, then Professor of Chemistry at Leipzig. But, in spite of much early opposition and neglect, the new point of view gradually triumphed. The warm support of Wislicenus, and the work of himself and his school, largely contributed to this consummation. Thus was born the now flourishing science of Stereochemistry.

In 1876 van't Hoff was appointed to the post of Docent in Physics in the State Veterinary School at Utrecht. During this period he occupied himself with a number of problems in organic chemistry, many of them relating to points connected with his new theory. As a result of his studies on the nature of the carbon atom and its compounds, there appeared (1878 and 1881) his highly original 'Ansichten über die organische Chemie,' in which he strove after a systematic arrangement of carbon compounds according to their structure, and emphasised the importance of studying chemical reactions from a kinetic point of view. Although preceded here by such pioneers as Harcourt and Esson, and Guldberg and Waage, he developed in the 'Ansichten' the fundamental equations of chemical kinetics and equilibrium on the basis of the law of mass-action.

In 1878 van't Hoff was appointed Professor of Chemistry, Mineralogy, and Geology in the newly created University of Amsterdam. Together with a number of pupils, he now began to develop with extraordinary insight and experimental skill the field of research already indicated in the 'Ansichten.' Starting with the object, "connaître les grandeurs précises et caractéristiques nécessaires pour comparer les propriétés chimiques d'un corps avec sa formule de constitution," he was led step by step to a complete and precise formulation of the velocity and course of chemical reactions and the influence of temperature thereon. At the same time, he applied the laws of thermodynamics with great success to the problems of chemical equilibrium and affinity. The first fruits of these labours appeared in book form in his classical 'Études de dynamique chimique' (1884). In the first section, van't Hoff gives a systematic account of the principles of chemical kinetics, with many new applications to special problems, and a new and important method of determining the number of molecules taking part in the reaction which controls the speed. The second section deals with the application of thermodynamics to chemical equilibria. The notion of "condensed systems" (in which none of the phases possesses variable composition) and the corresponding equilibria at definite temperatures (transition-points) are here clearly enunciated and experimentally treated. The distinguishing feature of this part is, perhaps, the clear and simple exposition of the relation between the change of internal energy of a reaction and the variation of the equilibrium-constant with temperature. This led van't Hoff to his "Principe de l'Équilibrie Mobile," which states that a rise (or fall) of temperature will displace the equilibrium in such a way as to favour that system whose formation is attended with an absorption (or evolution) of heat. This theorem may be regarded as a special case of a more general principle developed by Le Chatelier in the same year (1884).

In his discussion of these matters, van't Hoff showed in a masterly manner how the approximate validity and real practical value of Berthelot's principle could be reconciled with the accurate deductions of thermodynamics.

The third and perhaps most original part of the 'Études' deals with the definition and measurement of chemical affinity. Van't Hoff proposed to

measure the affinity with which substances combine or react by means of the (maximum) work obtainable when the reaction is conducted isothermally and reversibly. He showed how this work could be calculated not only from measurements of osmotic and gaseous pressures, but also from the electromotive force of reversible galvanic cells, thus, together with Helmholtz, laying the practical foundations of the newest chapter of modern electro-chemistry, namely, the electrometric measurement of the affinity of chemical reactions. In 1896 the 'Études' was published in a revised and much extended form by E. Cohen, one of van't Hoff's most distinguished pupils, under the title 'Studien zur Chemischen Dynamik.'

Already in 1884 van't Hoff's mind was busy with the idea of osmotic pressure. As he has himself related, his attention was drawn to the osmotic measurements of Pfeffer by his colleague, de Vries, Professor of Botany at Amsterdam. It occurred to van't Hoff that by the use of semi-permeable "osmotic" pistons he could apply the methods of Carnot to solutions. The important question then arose as to how the osmotic pressure was related to the temperature and concentration of the solution. The data of Pfeffer showed that for dilute solutions of cane sugar the osmotic pressure was very approximately equal to the pressure which the cane sugar would have exerted if it could have occupied as a gas the same volume at the same temperature.

Such was the origin of van't Hoff's famous "Theory of Solutions" (1886). But in order to include electrolytes he was obliged to introduce certain coefficients (i), the general equation taking the form $PV = iRT$. As is well known, these i-coefficients have received, in the case of dilute solutions, an interpretation by means of Arrhenius' theory of electrolytic dissociation.

In this way van't Hoff was enabled to work out a simple method of applying the laws of thermodynamics to many important problems, such as the relations between the freezing-point and boiling-point of dilute solutions and the corresponding molecular concentrations, the law of chemical equilibrium and its variation with temperature, the variation of solubility with temperature, etc. These investigations not only provided a sure basis for the methods of determining molecular weights in solution, but gave a great impetus to the study of chemical reactions and chemical equilibrium in (dilute) solutions. Van't Hoff's work was of the greatest influence in the development of the theory of electrolytic dissociation and modern electro-chemistry. Although preceded here by Gibbs and Helmholtz, he worked out the fundamental relationship between the equilibrium-constant of a chemical reaction and the electromotive force and concentrations of the constituents in a reversible galvanic cell in which the same reaction occurs.

During the period 1887—1895 van't Hoff published a considerable number of papers dealing with various points connected with his theory of solutions and its relation to the then rapidly developing theory of electrolytic dissociation. One of the most interesting and original of these, entitled "Über feste Lösungen und Molekulargewichtsbestimmung an festen Körpern"

(1890), opened up a new field of research. During the same period van't Hoff, together with a number of his pupils, was actively engaged in the investigation of the conditions determining the formation and decomposition of double salts. These researches were published in collected form in 1897 with the title 'Vorlesungen über Bildung und Spaltung von Doppelsalzen.' His treatment of the subject is characterised from the theoretical side by the application of thermodynamics, and from the experimental side by the elegant methods—microscopic, thermometric, dilatometric, tensimetric, and electrical—which he and his pupils worked out for the determination of the transition-points of the systems investigated.

As may be judged from the foregoing very brief survey the eighteen years, from 1878 to 1896, during which van't Hoff held the Professorship of Chemistry at Amsterdam, were years of fertile and many-sided research. After having refused several other calls, he accepted, in 1895, an invitation from the Prussian Academy of Sciences, who had elected him a member, to go to Berlin and establish a research laboratory there. He went to Berlin in 1896, becoming at the same time a Professor of the University. There, in collaboration with his old pupil, Meyerhoffer, and assisted by a small number of research students, he began that great series of investigations on the formation of oceanic salt deposits (with special reference to the salt beds at Stassfurt) which occupied him for more than ten years and inaugurated a new era in the study of experimental mineralogy. These researches were a logical outcome of the theoretical and experimental methods summarised in the 'Bildung und Spaltung von Doppelsalzen.' Van't Hoff's method consisted in determining the fundamental non-variant equilibria (consisting of vapour, solution, and three solid phases) which characterise a four-component system at each particular temperature. In this way he succeeded in systematically mapping out the whole region of investigation, so that the amount and nature of the various substances which can crystallise out under given conditions could be deduced. The results of these investigations were published in collected form with the modest title 'Zur Bildung der Ozeanischen Salzablagerungen' (in two volumes, 1905 and 1909).

The last few years of van't Hoff's life were clouded with illness, his lungs having become affected. But in spite of weakness due to the progress of the disease, and of enforced rests in sanatoria, he continued his scientific work. In his last laboratory, set amongst the pine trees of the royal demesne of Dahlem, between Berlin and Potsdam, he had planned and already begun an investigation of the action of enzymes, when death overtook him. He died at Steglitz on March 1, 1911.

During his residence in Berlin van't Hoff found time to publish in book form the lectures which he delivered at the University, with the title 'Vorlesungen über Theoretische und Physikalische Chemie.' The short series of lectures which he delivered in 1901 at the University of Chicago, on the occasion of the decennial celebrations of its foundation, were published

with the title 'Acht Vorträge über Physikalische Chemie' (1902). Both these works are characterised by remarkable breadth of outlook, combined with extreme conciseness of statement and close relation of theory to experimental results.

During his lifetime van't Hoff was the recipient of scientific and academic honours from all parts of the world. In 1888 he was elected an honorary Foreign Member of the Chemical Society of London. A similar honour was conferred on him by the Royal Society in 1897, and by the Physical Society in 1911. In 1889 the German Chemical Society, and in 1895 the German Bunsen Society elected him to honorary membership. He was the recipient of the Davy and Helmholtz medals, and of the Nobel prize for chemistry (1901). The Emperor of Germany conferred on him the Order "Pour le mérite," whilst honorary degrees were received from the universities of Cambridge, Chicago, Greifswald, Heidelberg, Manchester, and Utrecht. The thirty-first volume (1899) of the 'Zeitschrift für physikalische Chemie' (of which journal he had been joint editor since its foundation in 1887) was dedicated to him by his pupils in honour of the twenty-fifth anniversary of his promotion to the degree of Doctor.

In 1878 van't Hoff married Miss Jenny Mees, of Rotterdam, who survives him. By her he had two sons and two daughters, all of whom are living.

In his manner van't Hoff was simple and homely. To those who had the privilege of working with him he was endeared by the unaffected friendliness and sincerity of his nature. As a lecturer he made no pretence of oratorical brilliance. He was content to give his hearers an unadorned though profound and fundamental account of the development of chemical facts and theories. As an investigator he will ever be remembered as a great and outstanding genius. Although preceded by many great pioneers, such as Harcourt and Esson, Guldberg and Waage, Horstmann, Gibbs, Kirchhoff, Kelvin, Helmholtz, etc., it was van't Hoff who so developed and systematised chemical dynamics and thermodynamics, that he may well be regarded as one of the chief founders, if not the chief founder, of modern physical chemistry.

Regarding nature with the delicate and finely attuned perception of genius, he saw in chemical reactions phenomena whose course was subject to exact law, and whose limits were controlled by the play of affinities that could be measured and compared. The influence of his work extends far beyond the ordinary bounds of chemistry. Physiology and biology, dealing with the mysterious mechanism of living matter, in which dilute solutions, semi-permeable membranes and subtle chemical affinities play an important part, have received a powerful stimulus. A new vista of possibilities has been disclosed to geology and mineralogy. Everywhere the influence of van't Hoff's work has led to advance in the direction of quantitative relationship, to a profounder perception of causality in the sequence and balance of chemical phenomena.

F. G. D.

JOHN ATTFIELD, 1835—1911.

John Attfield was born in August, 1835, near Barnet, in Hertfordshire, the son of John Attfield, surveyor, of Whetstone. The name—originally At-the-fields, and later Atte Felde or Atefeld—is purely English, and it is therefore easy to trace the descent of John Attfield from the John Atefeld who flourished in "the Ville of Staundon" (now Standon, eight miles north-east of Hertford) as far back as 1361.

Attfield was educated at the school of the Rev. Alexander Stuart, then of Barnet, where he developed a taste for scientific pursuits as a direct result of the teaching he received. Having expressed a desire to be allowed to continue his studies in chemistry and physics, he was apprenticed, at the age of 14, for five years to Mr. William Frederick Smith, a pharmacist of Walworth. During the last year of his apprenticeship, 1854, he attended the School of Pharmacy of the Pharmaceutical Society in Bloomsbury Square, and obtained the first prizes or medals in all subjects—namely, chemistry, pharmacy, materia medica, and botany. He passed the Minor or qualifying examination of the Society in the same year, and offered himself as a candidate for the Major examination, but was refused admission, as he was not of age. In September, 1854, he obtained the position of junior assistant to Dr. Stenhouse, F.R.S., lecturer on chemistry in the medical school at St. Bartholomew's Hospital, and subsequently became demonstrator of chemistry at the same hospital. Stenhouse was succeeded by Dr. (afterwards Sir Edward) Frankland, and Attfield remained with the latter as demonstrator, assisting him in research work, besides lecturing at the Addiscombe Military College, until 1862, when he was appointed director of the laboratories of the School of Pharmacy of the Pharmaceutical Society, and afterwards Professor of Practical Chemistry, which subject was given up by Prof. Redwood so that he might devote all his attention to the teaching of theoretical chemistry and pharmacy.

Shortly after his appointment Attfield went to Tübingen, where he obtained the degree of Doctor of Philosophy, presenting as his thesis a paper on "The Spectrum of Carbon," read before the Royal Society in June, 1862. Attfield remained Professor of Practical Chemistry at Bloomsbury Square till his retirement from public life in 1896.

He was elected a Fellow of the Chemical Society in 1862, and was a member of Council during the period 1874–8; was a Fellow, one of the founders of, and for several years a member of Council of the Institute of Chemistry; was elected a Fellow of the Royal Society in 1880; was an honorary member of the Pharmaceutical Society of Great Britain, and of some twenty other pharmaceutical colleges and societies all over the world.

Attfield was no less active in private than in public life; he took a keen interest in educational, philanthropic, social, and recreative movements in

general, and was one of the leading spirits in the Herts Natural History Society and in the Watford Fieldpath Association. He retired practically from all public work in 1896; and, although suffering under rather severe physical disabilities, he was able to enjoy life in a very quiet way, his garden and his books being specially a great solace to him. He died at Ashlands, Watford, on Saturday, March 18, 1911.

Attfield's published work deals almost exclusively with scientific pharmacy. During the time he was at St. Bartholomew's Hospital he wrote most of the chemical articles in 'Brand's Dictionary of Art, Science, and Literature,' and in the Arts and Science Division of the 'English Cyclopædia,' and he also found time to revise and extend the chemical portion of the fourth edition of Clegg's work on 'The Manufacture and Distribution of Coal Gas.' His first original paper, "On the Solubility of Mercurial Precipitates in Alkaline Salts," was read in November, 1859, to the Chemical Discussion Association of the Pharmaceutical Society, and was published in the 'Chemical News' for 1860. From that time till 1897, he contributed between seventy and eighty papers to pharmaceutical and scientific literature.

In 1867 appeared the first edition of Attfield's 'Manual of Chemistry: General, Medical, and Pharmaceutical,' the basis of the work being some manuscript notes which the author had prepared for the students at St. Bartholomew's Hospital. The book has now reached its nineteenth edition.

Undoubtedly the greatest work of Attfield was in connection with the 'British Pharmacopœia.' It is interesting to note that the name Johannes Attfield appeared in the prefatory pages of the 'Pharmacopœia' of 1677, and of the reprints of 1678 and 1682, as one of the Fellows of the College of Physicians responsible for the production of the volumes, and he appears to have belonged to one and the same family as the editor of the 1898 'Pharmacopœia.' On the appearance of the 1864 edition of the 'Pharmacopœia,' lectures were given at Bloomsbury Square by Profs. Bentley, Redwood, and Attfield, which with other criticisms aided in the suppression of the book and the appointment by the General Medical Council of Prof. Redwood and Mr. Warrington to edit a new edition. Redwood was also asked to edit the 1885 edition, but declined, and Attfield and Bentley became associated with him, and on the completion of the work Attfield was appointed Reporter in Pharmacy to the Pharmacopœia Committee and Editor of an Addendum to the 'Pharmacopœia.' In carrying out this work Attfield succeeded in bringing about the recognised co-operation of the General Medical Council and the Pharmaceutical Society and the imperialisation of the 'Pharmacopœia,' as evidenced by the publication, under his exclusive editorship, of the 1898 edition and the Indian and Colonial Addendum, his success being largely due to the nine Annual Reports (1886–94) "On the Progress of Pharmacy in Relation to the 1885 British Pharmacopœia," prepared by him for the General Medical Council. The reports, which aptly illustrate Attfield's method and thoroughness, show how much the medical profession was

indebted to pharmacists for their voluntary efforts to make the 'Pharmacopœia' a better book. Attfield received the thanks of the General Medical Council "for all that he had done to make the 'Pharmacopœia' complete and accurate."

One of the principal projects in which Attfield was interested, and of which he was one of the founders, was the British Pharmaceutical Conference, which has for its object the promotion of pharmaceutical research and of good fellowship among its members. The annual meetings of the Conference, inaugurated at Newcastle in 1863, have always been very successful, owing largely to Attfield's influence, in appreciation of which the members presented him with 500 volumes of general literature in 1880 on his retirement from official connection with the Conference as its Honorary Secretary. In addition, this Association has ever since its foundation published a 'Year-Book of Pharmacy,' containing not only a full report of its meetings, but a digest of the scientific work bearing on Pharmacy published in other countries. The editorship of this publication was entrusted to Attfield and retained by him for many years.

Attfield's views on education were always of the broadest type. He had a high opinion of the value of chemistry as a means of culture, considering that it taught the student—to quote his own words—"to observe accurately, reflect accurately, and describe accurately on all and any matters in general life."

He was a strong advocate of a curriculum of study, regarding the acquisition of knowledge merely for examination purposes as pernicious; hence he questioned any method of examining candidates which did not take note of the quality of the educational course which had been gone through. As a teacher he was pre-eminently kind and sympathetic, and the esteem in which he was held by his students may be gauged from the following extract from an address presented to Attfield in July, 1897, together with a silver tray and silver tea and coffee service, by his old pupils and friends:—

"During the whole of this long tenure of his important office Prof. Attfield not only won and retained the respect of successive generations of students by the lucidity, accuracy, and thoroughness of his teaching, but he also endeared himself to them by his unfailing tact, kindness, and urbanity. Not less successfully did he serve pharmacists and medical practitioners, and through them the public, by his versatile ability, untiring energy, and power of organisation as an editor of the 'Pharmacopœia,' and author of a manual of chemistry, and generally as a worker who unceasingly applied the resources of the great science of chemistry to the demands of the great art of healing."

A. C.

NEVIL STORY-MASKELYNE, 1823—1911.

In view of the near approach of the 250th Anniversary of the Royal Society, it is worthy of remark that the death of Mervyn Herbert Nevil Story-Maskelyne removes from the roll of living Fellows the name of one whose father and grandfather were Fellows before him. With a short break between 1811 and 1823 these three generations held the Fellowship from 1758 to 1911, that is, for 140 out of 153 consecutive years, or for more than half the whole period during which the Society has existed.

The Maskelyne family can be traced back as landowners in Wiltshire to 1435. Though the returns for the borough of Cricklade (1625) are missing, Brown Willis states that Edmund Maskelyne (1564—1629) was M.P. for Cricklade in that year, and the same constituency was also represented by his son, Nevill Maskelyne, in 1660, and again (both before and after its conversion into the Cricklade Division of Wiltshire) by the late Mr. Story-Maskelyne from 1880 to 1892.

The introduction of the name Nevill or Nevil into the family arose from the fact that the wife of Edmund Maskelyne was grand-daughter of Mary Neville, sister of Lord Abergavenny. It was borne by many of Edmund's descendants, among whom was the celebrated Astronomer Royal. It is unnecessary to recapitulate here the claims to distinction of this well-known Copley Medallist. His only child, Margaret Maskelyne, married in 1819 Mr. Anthony Mervyn Reeve Story, who had taken a Double First at Oxford at the early age of 19, and was elected a Fellow of the Royal Society in 1823. Subsequently, Mr. Story, as owner of the Maskelyne estates in right of his wife, took the name of Story-Maskelyne, by which his descendants were thenceforth known.

His eldest son, the subject of this memoir, was born on September 3, 1823; was educated at Bruton Grammar School, in Somersetshire; entered Wadham College, Oxford, in 1842; and took his Degree with a Second Class in Mathematics in 1845. It is probable that his taste for experiment prevented his attaining higher honours, for, as an undergraduate, he had taken to the study of chemistry, which he himself sarcastically described as "an absolutely useless or rather harmful study, as distracting the mind from the degree subjects of the Schools."

He had been intended by his father for the Bar, and, after leaving Oxford spent his time between Basset Down, the family place in Wiltshire, and the Temple. The story of how science ultimately captured him is a sufficiently good illustration of the difficulties with which students of science had to contend in the forties and early fifties of the last century to be worth telling in some detail.

About two years before he went to Oxford Story-Maskelyne had become known to Dr. Buckland, who visited Basset Down to collect fossils in the

great cutting in the Kimmeridge clay made between Swindon and Wootton Bassett during the construction of the Great Western Railway. By him the young man, shortly after his arrival in Oxford, was introduced to Dr. Daubeny, and thus was welcomed to the meetings of the Ashmolean Society, to which, as he believed, he was the only undergraduate then admitted. His impressions of these meetings were described by him in an autobiographical note which he wrote in November, 1908, in response to a letter from Mrs. Gordon, a daughter of Dean Buckland:—

"The discussions," he said, "were sometimes interesting, often not very interesting, to me at least, but as I felt even then, and have always felt since, these gentlemen held up the lamp of learning in natural science that had been lit more than a century and a half before by Boyle and Wren and other founders of the Royal Society. The lamp was flickering, but it has since burnt more brightly under the new conditions of its feeding."

During his undergraduate days, as has been already stated, he studied chemistry, attending Dr. Daubeny's lectures in that subject, Buckland's in mineralogy, and experimenting both in his own rooms and later in a laboratory which he established at Basset Down.

He also formed an early intimacy with Fox-Talbot, a Wiltshire neighbour, whom he frequently visited, and who was attracted by the fact that Maskelyne himself was among the first to practise the art of photography in its then stage of development. At the meeting of the British Association held in Oxford in 1847 the latter read a short paper on some of his results, and he used to relate that he showed the method of developing a photograph to Faraday, who had never seen the operation performed before. At a later period Maskelyne was recognised as an authority on the early history of photography. An anonymous article on "The Present State of Photography," which appeared in the 'National Review' in April, 1859 (No. 16, pp. 365—392, Chapman and Hall), was written by him, and he was Secretary to a Committee which reported to the British Association on the same subject in the same year ('Rep.,' 1859, pp. 103—110).

To return, however, to the "forties," Dr. Buckland was made Dean of Westminster in 1845, the year that Story-Maskelyne took his degree. While reading at the Temple the youth was a frequent visitor at the Deanery, where he met many well-known men, among whom was Sir David Brewster. How favourable the impression he produced must have been is shown by the fact that Brewster, then Principal of the University of St. Andrews, invited him to become a candidate for a Professorship there, but as the recipient of the compliment ambiguously said, "guided by parental wisdom, or in obedience to its authority, I continued my studies in a conveyancer's chambers."

But neither wisdom nor authority could for much longer restrain the young chemist from yielding to his natural bent. At the meeting of the British Association already referred to (1847) he met Benjamin Brodie, who had just returned to London after completing the Giessen course under

Liebig. This new friend, who offered the hesitating recruit an opportunity of working at chemistry in his own laboratory, induced him to give up law and to throw in his lot with science, a decision which was doubtless strengthened by his growing friendship with Faraday. Only three years later, in 1850, Dean Buckland became unable to carry on his work at Oxford, and, to his great surprise, Story-Maskelyne was asked if he would undertake the lectures on Mineralogy which had been delivered by his old friend. With becoming modesty he suggested that Brodie should have been invited in preference to himself, but when Brodie refused the invitation he consulted Faraday as to his own course of action.

"I told him," he afterwards wrote to Mrs. Gordon, "that I had collected minerals at one time and had only superficially studied them, but had no fear as regards the subject of mineralogy alone; but that crystallography was essential as the most important part of the subject, and with that I had only the slight acquaintance that the ordinary chemist was equipped with. Faraday's answer was, 'Accept the offer, and, as you have several months before you, come here (to the Royal Institution) on such evenings as you may have [free]. You shall have a room and light, and I will get from the library any books you may need.' A noble offer, need I say that I accepted it?"

His acceptance of the Oxford post was, however, subject to the condition that a laboratory should be assigned to him where he could teach Mineralogical Analysis and Chemistry in general. In answer to this request a suite of rooms was allotted to him, situate under the Ashmolean Museum and comprising six living rooms and offices, a laboratory, and a small theatre. There he lived from 1851 to 1858.

Previous to the opening of his laboratory, chemical manipulation had not been taught in the University of Oxford, and great interest was excited by the opportunity of learning what sort of thing modern chemistry might be.

The first applicant for admission was Thompson, afterwards Archbishop of York, and he was followed by Henry Stephen Smith (afterwards the well-known Savilian Professor of Geometry), Richard Congreve, Charles Pearson, and many others.

During his residence in Oxford Story-Maskelyne served as Secretary of the Ashmolean Society, and published various papers, among which may be mentioned that "On the Oxidation of Chinese Wax" ('Chem. Soc. Journ.,' vol. 5, 1853, pp. 24—26), and an "Investigation on the Vegetable Tallow from a Chinese Plant (*Stillingia sebifera*)" ('Chem. Soc. Journ.,' vol. 7, 1856, pp. 1—13; Erdmann's 'Journ. Prakt. Chemie,' vol. 65, 1855, pp. 287—296).

In addition to his scientific work Story-Maskelyne took an active part in the historic struggle for the erection of a museum in Oxford.

"There were two Delegacies," he wrote, "to which the museum question was entrusted. I was Secretary to the first Delegacy, Charles Conybeare, of Christ Church, an excellent man and an ardent supporter of the cause, was Secretary to the second Delegacy. I had collected in London a quantity of

suggestions for the mode of lighting, size of rooms, etc., at the desire of the Delegacy, and the general scheme was sketched out sufficiently for a further step in discussing the actual [erection of the building]. This matter went before Convocation, and a big meeting was held in the theatre . . . [but] the proposal was thrown out. . . . Much unpleasant feeling was produced, and I think some who were in opposition became convinced they had made a mistake."

This defeat was the precursor of ultimate victory, but it is needless to tell again the tale of how, under the leadership of Sir Henry Acland, that victory was won.

In 1855 (a date wrongly given as 1865 in the new edition of the 'Encyclopædia Britannica,' p. 625) Sir Benjamin Brodie was appointed Professor of Chemistry at Oxford, and Story-Maskelyne, who was successively Deputy Reader, Reader, and Professor of Mineralogy, held also for a short time the title of Assistant Professor of Chemistry. By Brodie's appointment, however, as Maskelyne himself said, his own Chemical Laboratory became "redundant" as a University Institution. The difficulty was soon solved, for within the next two years he was appointed to the newly-established post of Keeper of the Minerals at the British Museum, in which, perhaps, the chief work of his life was done.

He continued to hold the Chair of Mineralogy at Oxford, and by inviting the most promising of his pupils to work with him in London he extended the usefulness of both offices, and trained the next generation of British mineralogists. Among the best known of these are W. J. Lewis, the Professor of Mineralogy at Cambridge; Lazarus Fletcher, who succeeded him as Keeper of the Minerals, and is now Director of the Natural History Museum at South Kensington; and Sir Henry Miers, who followed him as Professor at Oxford, and is now Principal of the University of London.

To these may be added Dr. Viktor von Lang, who, after studying for two years in the British Museum, was successively Professor of Physics at Grätz and Vienna, and was associated with Joseph Grailich in a series of Memoirs on "Crystallography," published in the 'Sitzungsberichte' of the Academy of Vienna.

On settling in London Mr. Maskelyne married a lady who was herself a member of a scientific family, being grand-daughter of Dillwyn, the well-known botanist, and daughter of Mr. John Llewelyn, of Penllergare, Swansea, who was a Fellow of the Royal Society.

For six years previous to 1857 there had been no one at the British Museum who took any special interest in Mineralogy, and it was only in that year that the special Department of Mineralogy was instituted, and Story-Maskelyne was appointed Keeper. For five years he had only one assistant, the late Mr. Thomas Davies, who came without any special training, but, fortunately, developed an extraordinary capacity for museum work, acquired an unrivalled eye-knowledge of minerals, and became a most valuable scientific assistant.

Maskelyne undertook the whole rearrangement of the collection according to the crystallo-chemical system of Rose. He busied himself much with the acquisition of specimens, and, under his direction, the collection was enormously increased and improved. He was quick to see the importance of making as complete a collection of meteorites as possible, and devoted a great deal of his time and energy to their study.

There was at that time no laboratory or equipment for scientific research. Gas was not allowed in the building, and it was therefore extremely difficult to conduct any real scientific investigation. Maskelyne's work was during these years very largely confined to what could be done with the microscope and goniometer. He was one of the first to investigate thin sections of rocks and minerals with the aid of polarised light, and was actually using a microscope, with a revolving stage and eyepiece micrometer, for the study of thin sections of meteorites, about the year 1861.

His goniometer work was carried on with great difficulty without the assistance of proper artificial light, and he always attributed the deterioration of his eyesight to the strain of this work. That he, nevertheless, managed to carry out difficult and minute crystal determinations either at the Museum or at home, is shown by the remarkable measurements made on almost microscopic crystals of Connellite, and on those of Asmanite.

By 1867 he had made it clear to the authorities that scientific research could not be carried on without a chemical laboratory, and in that year Dr. Flight was appointed as assistant, and the laboratory was fitted up in a house outside the Museum.

Maskelyne held the office of Keeper of Minerals from 1857 to 1880, and, during that period, was able not only to maintain and develop the collections, so that they became the largest and best arranged series of minerals and meteorites in existence, but also to issue, with the help of his assistants, an important series of scientific memoirs.

Among these especially noteworthy are the series of 'Mineralogical Notes' published in 1863–4, in conjunction with von Lang, and the 'Mineralogical Notices' published in 1871–4, in conjunction with Flight.

It is stated in an obituary notice in the 'Mineralogical Magazine' that, during Maskelyne's tenure of the Keepership, no less than 43,000 specimens were added to the collection. These included, among others, the well-known Koksharov Collection acquired in 1865, in connection with which Maskelyne paid a visit to Russia, in order to negotiate the purchase, and the Allan-Greg Collection (1880), which included a fine series of British minerals well catalogued.

A survey of Maskelyne's published papers shows that, up to 1855, he was chiefly interested in chemical problems, that the study of meteorites engaged his attention as soon as he went to the British Museum, and that the period of his greatest scientific activity, as a mineralogist, judged by the published papers on minerals and meteorites, ranged from 1860 to 1880.

Among the more important of the papers on meteorites may be mentioned

the investigations on the Parnallee, Nellore, Breitenbach, Manegaum, Busti, Shalka, and Rowton meteorites; among mineral researches, those upon Langite, Melaconite, Tenorite, Andrewsite, Connellite, Chalkosiderite, and Ludlamite.

New minerals described by him were Andrewsite, Langite, Liskeardite, and Waringtonite, and the following constituents of meteoric stones were first isolated and determined by him: Asmanite, described in 1871 as an orthorhombic variety of silica from the Breitenbach meteorite isomorphous with Brookite, but now generally regarded as identical with the mineral Tridymite, described by vom Rath in 1868; Oldhamite, a calcium sulphide, described in 1862, from the Busti meteorite; and Osbornite, described in 1870, from the same stone. He was also the first to recognise the presence of Enstatite in meteorites.

He was always much interested in the history of the diamond, and, in a paper published in 'Nature' in 1891, returned to the history of the Koh-i-noor, on which he had first written in 1860. There was considerable stir about 1880, just before he left the Museum, concerning the artificial production of the diamond; Maskelyne proved that the supposed diamonds which had been manufactured by Mactear were in reality a crystallised silicate.

The mode of occurrence of the diamond in South Africa also occupied his attention, and he described the Enstatite rock, which is associated with it in that part of the world.

Maskelyne's views on Crystallography were first made public in a series of lectures delivered before the Chemical Society in 1874, and more fully in his well-known text-book, 'The Morphology of Crystals,' which was published in 1895. In this, as in his University lectures, the attention of students was especially called to the important subject of symmetry. From his early days this had particularly engaged Maskelyne's attention, and many of his views are no doubt the result of discussions between him and Viktor von Lang at the time when the latter was Assistant in the Mineral Department, from 1862–4. Von Lang's book, which was published in 1866, treated the subject of crystal symmetry in a novel and original manner, and Maskelyne's publications were devoted to the development of this subject on somewhat similar lines. His 'Morphology of Crystals' was published at so late a date that other methods of treatment, and another nomenclature, had become somewhat firmly established before it appeared. Had it been published when first written, it would have attracted much attention as a highly original work. Some of the proof-sheets were in the hands of his pupils for very many years before the book ultimately appeared; his views had been made known to them throughout the whole period of his professional career, and profoundly influenced their own teaching.

The chief feature of this book is the investigation of the geometrical relations of a crystalloid system of planes, that is to say, planes obeying the law of rational indices, and the most important and original part of the investigation is that which deals with the varieties of symmetry possible in

such a system. The subject is developed by a study of the symmetry planes inherent in the system; the important proof concerning the number of symmetry planes possible in a crystalloid system and their mutual inclinations was, as is stated by Prof. Lewis in his 'Crystallography,' given in a course of lectures by Prof. Maskelyne as far back as 1869. The whole treatment of the subject is elegant and lucid, and the new and expressive nomenclature employed invests these chapters with a certain charm, but by the time the book appeared other investigators had begun to approach the subject of crystal symmetry from another point of view. The independent operation of axes and planes of symmetry, and the application of these operations to homogeneous molecular structures, had been adopted as a less arbitrary and more fundamental treatment. Maskelyne contemplated a second volume, which was to deal with the physical properties of crystals, and had this been written he would doubtless have elaborated and criticised the newer methods. Some portions of this book were for several years actually in print, but he never found himself sufficiently free to complete the work.

In his mathematical, as in his scientific, writings Maskelyne's work was characterised by a remarkable distinction and charm of style, which was, indeed, part of his character, and prevailed in everything that he undertook. In all his scientific work the mind and sympathies of an artist declared themselves, and were as well known to his pupils as to his personal friends.

It was therefore characteristic that his devotion to his work as Keeper of Minerals did not lead him to narrow the range of his interests to mere specialism, but to extend it to a kindred subject. A good deal of his scientific work brought him into contact with ancient art, for which he always had a great fondness, and which was fostered by his opportunities in the British Museum. In his own words "Antique gems and Greek art got hold of me as I went continually through the galleries of the British Museum on my way to my own." But though favourable circumstances undoubtedly stimulated him, his taste was shown both before and long after he was connected with the Museum. As far back as 1869, for example, he was discussing at the Society of Antiquaries the nature of the Murrhine vases of the ancients, which he believed to be composed of heated sard, in opposition to Westrop, who thought Pliny's Murrhina to be fluor. As late as 1894 he was lecturing in Wiltshire upon the subject of Greek art. In the intervening years he not only formed a very valuable private collection of antique gems, but, at the request of the Duke, produced the well-known catalogue of the engraved gems belonging to the Duke of Marlborough's collection.

A great change took place in Maskelyne's life on the death of his father in 1879. Henceforth he became an active country gentleman, though he continued to hold his office of Professor of Mineralogy at Oxford. At that date funds were not available for securing the whole time of a resident

professor, and it was in the interests of the University itself that he retained office while waiting for the adequate endowment of his Chair. So soon as this was secured, by attaching the professorship to the Waynflete foundation at Magdalen College, he retired.

In 1880 he was elected in the Liberal interest as Member for Cricklade, and thereafter sat in three Parliaments, *i.e.* until 1892. Though he could hardly be called a conspicuous, he was unquestionably an active Member of Parliament. He was one of the Liberals who refused to follow Mr. Gladstone in his Home Rule policy, and without any doubt or hesitation ranged himself from the first among those who threw out the Bill of 1886. His constituency, nevertheless, returned him again to Parliament in the General Election which followed. It is not too much to say that the maintenance of the Union and of the efficiency of the Navy were two of the leading articles of his political creed. He served on several Committees, such as that concerned with the subsidences in Cheshire due to salt workings, and as a Member of the Committee on Electric Lighting he always opposed what he believed to be the harmful obstacles then thrown in the way of the development of the nascent industry. He was one of the members who supported the Commons Preservation Society, and this led to his Chairmanship of the Committee that dealt with the Thames Preservation Bill. His interest in politics did not flag after his defeat in the election of 1892, and was enhanced by the career of his son-in-law, the late Mr. Arnold-Forster.

He also took an active part in county matters, was a Member of the Wiltshire County Council from its foundation till he was over eighty years of age, and was for many years Chairman of the Agricultural Committee. He was an active Member of the Bath and West of England Agricultural Society, the presidency of which he declined, on account of his advanced years, when the meeting was held in Swindon. It was at his suggestion that the first itinerant dairy school was established.

In 1904 Mr. Maskelyne underwent a severe operation, and from that time till his death in 1911 he was always more or less of an invalid. His old age was, however, brightened by his intense mental activity and by his interest in the progress of science. In 1904, when his life was in great danger, he exclaimed: "I must live, I want to know more about radium."

He was a good scholar, and was one of the few scientific men who read Homer till late in life. The variety of his interests had brought him into contact with many of the most distinguished minds of two generations. He knew the bearers of all the best known Oxford names. He helped to entertain Liebig when he visited that University. He was the intimate friend of Charles Mansfield, the chemist, who died too young to have achieved a great popular name, but whose heroic end in a laboratory accident he could not recall without emotion. With him he worked in the social movement associated with the names of Kingsley and Maurice, and frequently lectured at the Working Men's College. At the British

Museum he was visited by many foreign mineralogists and collectors of minerals and gems. As a Member of Parliament he was on terms of friendship with Mr. Chamberlain and many other politicians. His wide knowledge of men and things, his memories of a long distant past, and his keen attention to the problems which are interesting the youth of to-day, made him a delightful companion to young and old.

To use his own words, "the twilight of his departing day" was cheered by the consciousness of having lived through a great age and by the pleasure of having "known, in different degrees, so very many of the vigorous men to whom that era is indebted for its splendour."

Mr. Story-Maskelyne was the recipient of many testimonies to his scientific worth. He was an Honorary Fellow of Wadham College, and received in 1902 the Honorary Degree of Doctor of Science from the University of Oxford. He became a Fellow of the Royal Society in 1870, served twice on the Council, and was Vice-President from 1897 to 1899. He was Corresponding or Honorary Member of the Imperial Mineralogical Society of St. Petersburg, of the Society of Natural History of Boston, of the Royal Academy of Bavaria, and of the Academy of Natural Sciences in Philadelphia.

Mrs. Maskelyne and his three daughters survive him. One of the latter married the late Right Hon. H. O. Arnold-Forster, some time Secretary of State for War, and another is the wife of Sir Arthur Rücker.

A. W. R.
H. A. M.

William Huggins

Photo by Miss Edis, 1910

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